A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. XXXI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1812.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson; Luke Howard, Esq.; Richard Lovell Edgeworth, Esq. F. R. S. M. R. I. A.; A. Z.; Mr. George John Singer; L. O. C.; Mr. Benjamin Cook; Mr. John Murray; A Lover of the Modern Analysis; James Clarke, M. D. late Physician to the Nottingham General Hospital; George Pearson, M. D. F. R. S. &c.; Mathematicus; the Right Hon. Lord Gray; Alexander Marcet, M. D. F. R. S. one of the Physicians to Guy's Hospital; A. B. C.; J. Phœnix; Benjamin Smith Barton, M. D. Mem. of the Am. Phil. Soc. &c.; J. D. Maycock, M. D.; John Davy, Esq.; and a Correspondent.

Of Foreign Works, M. Henry; M. Klaproth; Mr. J. E. Berard; M. Gay-Lussac; M. Guyton-Morveau; M. Theodore de Saussure; M. Bucholz; M. Hildebrandt; M. Reuss; M. Vauquelin; M. Huberle; M. Marcel de Serres; and Dr. Francis Delaroche.

And of British Memoirs abridged or extracted, Mr. J. Allan; William Thomas Brande, Esq. F. R. S.; James Parkinson, Esq.; Mem. of the Geol. Soc.; Mr. William Salisbury; William Hyde Wollaston, M. D. Sec. R. S; Alexander Marcet, M. D F. R. S. &c.; Mr. Adam Reid; Mr. George Spark; Thomas Andrew Knight, Esq. F. R S. &c.; Mr. John Maher, F. H. S.; A. Hawkins, Esq.; William Fitton, M. D.; William Charles Wells, M. D. F. R. S.; Mr. Benjamin Cook; Mr. H. B. Way; Smithson Tennant, Esq. F. R. S. &c.; and W. H. Pepys, Esq. F. R. S. &c.

The Engravings consist of 1. Delineations from Nature illustrative of the Mechanism of Leaves, by Mrs. Agnes Ibbetson. 2. Mr. Allan's Mathematical Dividing Engine. 3 and 4. Delineations from Nature of different Parts of Flowers, to illustrate their Mechanism, by Mrs. Agnes Ibbetson. 5. Figures to illustrate some Diseases of Vegetables, delineated from Nature, by Mrs. Agnes Ibbetson. 6. Appearances from Sugar in the Serum of Blood, by W. H. Wollaston, M. D. Sec. R. S. 7. A Compensation Pendulum, by Mr. Adam Reid. 8. Mr. G. Spark's Noctuary, or Apparatus for indicating the Hour in the Dark by Means of a common Watch. 9 and 10. Dissections of Aquatic and Semi-aquatic Plants, delineated from Nature by Mrs. A. Ibbetson. 11. Apparatus for making Gas and various other Products from Pit-coal, by Mr. Ben. Cook. 12. Method of procuring Turpentine from British Firs, by Mr. H. B. Way. 13. Apparatus for exposing Animals to a heated humid Atmosphere, by Dr. Francis Delaroche.

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259 Note, *for* 185, *read* 134.
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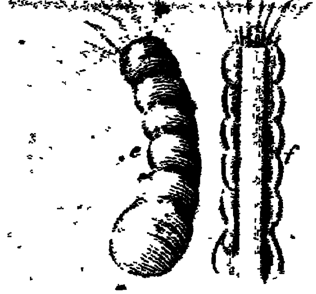
Mechanism of [illegible]

Fig. 7.



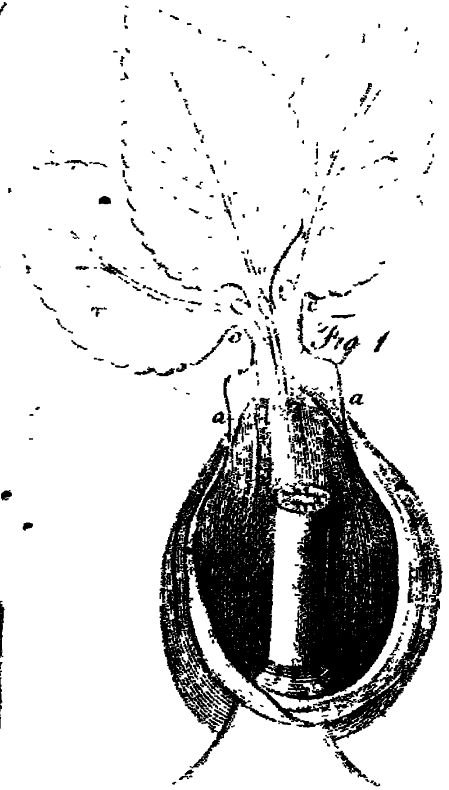
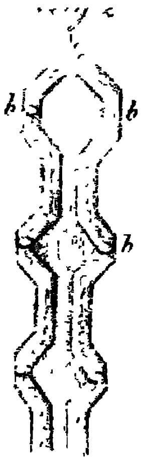


Fig 1

Fig 2



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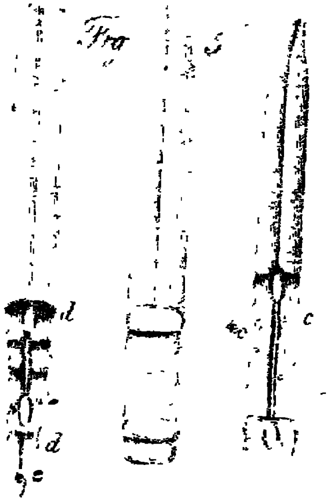

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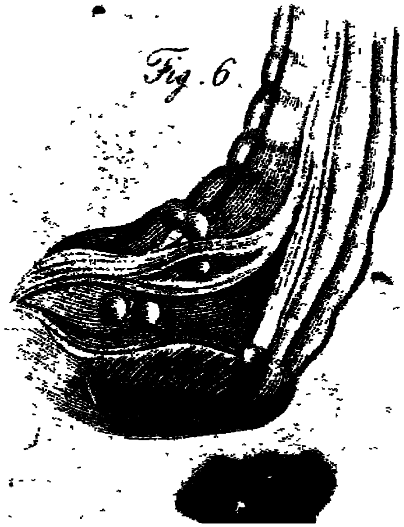


Fig. 3

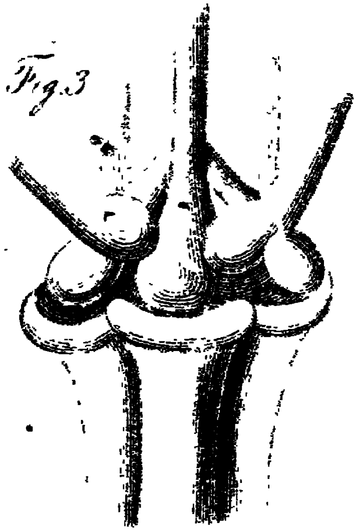
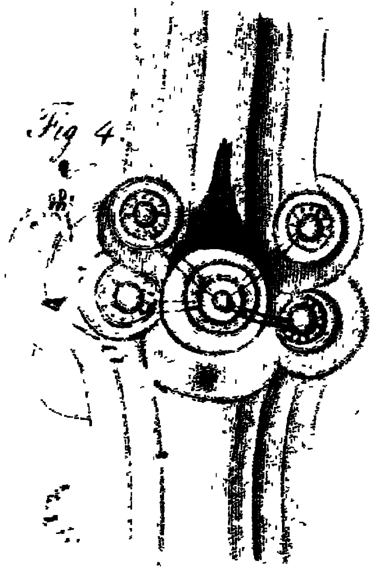


Fig 4



A
JOURNAL
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THE ARTS

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JANUARY, 1812.

ARTICLE I.

On the Mechanism of Leaves. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

TO prove so important a point, as that all plants are governed by mechanical means, it will not I hope be thought superfluous, to give a specimen of the sort of mechanism belonging to each part of a plant. I have already shown in what manner the mechanical power increases progressively from the firs, which have no spiral wire, to the sensitive plant, which has such a complicated arrangement. I have also shown the mechanical use of the gatherers of the leaves, whether one or two. I shall in the present letter therefore point out the means, by which leaves embracing the stem have also the power of turning and changing their position, equalling in this respect those which I described in my last letter: I shall next point out those stems which are formed like joints, and which may be said wholly to differ in mechanism from the former, turning on a ball and socket: these are most of the galia, arenariæ, stel-

Subject of the Letter.

larix, and also many of the twining plants: I shall then shew the manner in which most leaves that are nearly sessile are conducted, and what sort of motion they possess, giving as an example the ericæ; and shall finish by the evidence of a few more leaves, to prove, that there are hardly any without some sort of mechanism.

Mechanical arrangement of leaves embracing the stem.

To begin with the species of leaves, which embrace the stem, I shall produce as an example one of the umbelliferous tribe; the smyrnium olusatrum. This plant has at the end of the leaf a large hood, which I shall call the protector; because it not only contracts and dilates like a gatherer (the spiral wire running in stripes through it) but serves as a guard to the buds; which, forming in bunches, stay not within the bark, as the buds of trees, till fit to issue forth, but shoot, like all annuals and herbaceous plants, directly from the line of life to the exterior of the rind. Nature seems therefore to have placed this sort of cover (see Pl. I, fig. 1, *a a*) which grows with its growth, and clings close to it, as a succedaneum for the stem, which in trees covers and conceals the bud for a long time. And it can scarcely be conceived what a perfect protection it is from the frosts of spring, as, like all leaves, it has on each side that impervious skin or cuticle, which no rain or cold winds can pierce, and no moisture pervades, but that which passes through the hairs into the leaf, and that which evaporation gives it. Most of the pentandria digynia tribe are formed *thus*, at least with leaves possessing this species of mechanism. It may easily be seen how much the spiral wire is contracted and turns, since, whether *open* or *shut*, the protector is continually twisted in a double manner round the stem; nor does the large bunch of flowers leave its close drawn curtain, till just before the corollas open. Being umbelliferous, large branches of flowers shoot at once; as soon as the cover is withdrawn, new life as well as light seems to be given to the plant within, which is, when first opened, generally found covered with a white powder, probably the result of evaporation. Of this I shall hereafter give a farther account. But when the leaflets in this plant increase to a great length (which they will often do to ten or twelve leaves of a side) though they in some measure resemble

Pentandria digynia plants.

resemble the ash, they are governed by a very different species of mechanism: they have a swelling down each side of the stalk, in which the spiral wire runs in a groove (see fig. 2, *b b*) which corresponds with the sort of knob round which the wire of each leaf is turned; communicating all the way with a knot and pulley till it reaches the protector at *c c*, fig. 1.

Mechanism of the ball and socket.

The management of the galia, and most of those plants, the stalks of which enlarge where the leaves meet, is contrived in a curious manner, with a sort of mechanism consisting of a ball and socket. No part of a plant (the seed excepted) has given me so much trouble as this; having found two sorts of balls in vegetable life, and confounded the two together, viz. the ball and excrescence in trees, and the ball continually found appertaining to the mechanism of plants. I have at last learned to distinguish them. Of those in trees I shall soon give a description, when entering again on the subject of the wood in plants; which will develope many hidden secrets, that ought to be explained, and illustrate many things that may have appeared contradictory. Respecting the ball, which forms a part of the mechanism in plants, each stem has one, on which it turns like the knee of a quadruped: but, to see the ball, the plant must be taken at a very early age, for it is the first part that decays, and in the phaseolus vulgaris I have known it gone before the flower disappeared; and I have often seen five or six stems turn in one collected set of joints. When old they stiffen, and a sort of matter fills up the interstices of the sockets. They then become immovable; but before this, if you move the stem, you may see them turn, but cannot turn them yourself without breaking the spiral wire. See fig. 3, and a section at fig. 4. This specimen will at least discover the manner in which the spiral wire is conveyed from one ball to another, communicating its influence, and spreading its power from stem to stem.

The ericæ possess the spiral wire.

I must now correct a mistake I before made in saying, that there was no spiral wire in the ericæ. It is so diminutive, and lies so low in the groove, that I overlooked it. The leaves of the heaths, though very sessile, still possess a motion to and from the plant. See fig. 5, *c c*. They are placed so much in the manner of the fir leaves, that I was deceived by it. But in the firs the rind and bark are all leaves only, whereas in the ericæ the leaves are wholly detached, nor are they quite sessile, as may be seen at fig. 5, *d d*. I shall now show the manner in which almost all leaves that are evergreen, and have a shining surface, are formed in their peduncle, having but one gatherer, which embraces the whole length of the leaf-stalk. The upper part alone puckers, as the motion requires it; by this part to be gathered up, when it is to be raised close to the stem, or stretched out, when it is to fall back. See fig. 7, where *e* is the back and *f* is the front.

Bud of the leaf and when it will be seen.

I have already said, that almost all leaves of trees have one or two gatherers, if only one, it is *that adjoining the stem*, as it is *that* in which the bud of both are contained; nothing is more easy than to know the leaf from the flower bud, even before they leave the stalk, the leaf bud is so peculiar in its appearance it is impossible to mistake it: it is composed of a quantity of hairs, or vessels, which are already beginning to weave the leaf. It is more than four times the size of the flower bud, and in trees generally of a bright brown colour, very shining and beautiful. See fig. 6 *g g*. I hardly know a subject more worth studying in the solar microscope *than this*, if taken progressively from its first beginning to its leaving the stalk as a bud, for it is in this early state it shows the whole process of weaving the leaf, as it protrudes not from the stalk till the form is complete. Though Linnæus admired much the manner of pressing and folding the leaf, he was totally ignorant of its beautiful commencement. I have given the lower gatherer of the tilia, fig. 6. It is very curious to see, when the flower buds are many in number, (fig. 3, *h h*,) how the wood vessel and spiral wire will meander round them, that they may not injure by passing over them; for if, while resting on the buds, they were suddenly *contracted*, they would probably divide, and thus destroy them. I think I have now shown the mechanism of most leaves, my next letter will give the mechanical management of flowers.

I am, Sir, your obliged servant,

AGNES IBBETSON.

II.

II.

*Improvements in a Mathematical Dividing Engine by Mr. J. Allan, of Blewit's Buildings, Fetter Lane**.

SIR,

I beg leave to send to you, herewith, for the inspection of the Society of Arts &c., a model of my improvement on the mathematical dividing engine, which I have lately made, containing that part which differs in principle from those made by the late Mr. Ramsden and others; the drawings or engravings of which are, I suppose, in the Society's possession. I therefore am of opinion the Society will think, that the wooden wheel I have sent, with the movable ring on its edge, will be sufficient to demonstrate its good effect in correcting the teeth or rack where the screw acts. You will please to observe, that it is cut by a screw cutter, and it is required to go many times round the engine before the teeth are full. To effect this, I reversed the movable ring not less than twenty times, so that I have not the least doubt of the one ring having corrected the other to a degree of perfection, which had not hitherto been obtained in engines.

Improvement in mathematical dividing engine

This simple, easy, and correct way of making engines, may be applied with great advantage to circular instruments, for the purposes of astronomy and land surveying. If the Society will do me the honour to appoint a Committee to view the engine itself, I will demonstrate its effects.

I am, Sir,
Your very humble servant,
JAMES ALLAN,
Divider of Mathematical Instruments.

No. 12, Blewit's Buildings, Fetter Lane,
Nov. 20, 1809.

Mr. Allan's Description of his Mathematical Dividing Engine, and his Method of forming it.

My engine is of belmetal, thirty inches in diameter. I turned a brass ring about three sixteenths of an inch thick,

The engine described.

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* Trans. of the Soc. of Arts &c., vol. XXVIII, p. 179. The gold medal was voted to Mr. Allan for this improvement.

and fitted in on the underside of the above belmetal wheel, which I made fast by twenty four rivets; I then fixed in the axis, and turned the wheel and ring together on the lathe, as near as possible to the required shape on its own axis. This being done, and having mounted it on its own stand, where it now acts, I fixed a tool, with an adjustment to turn the edge of the belmetal wheel where the uppermost or movable ring of the same thickness as the other is fitted on; for if the circle, where the movable ring fits the belmetal, is not turned as true as possible, (which cannot be done properly by any other means than by a fixed tool) the movable ring will not reverse correctly. When this was done, I fitted on the movable ring. I then divided the lower under ring into twenty four parts, for the screws which keep the rings together. I also divided it into four parts for the steady pins, the holes of which I made by an upright drill fixed and adjusted for the purpose. I then cut two opposite divisions, in order to reverse the uppermost ring correctly, which were my guide in broaching for my steady pins, and which I did with a broach to a stop fixed on it. In broaching I reversed the movable ring many times, taking care at the same time that my opposite divisions were correct.

My first idea was to have two wheels or circles, acting on the same centre, so as to constitute a double edge, to afford me an opportunity to reverse in the act of cutting the rack or teeth, but I thought the method in which I have done it would with care be equally correct. Either of the methods come to the same point, and I preferred the way I have employed, thinking it the least expensive. By this self-correcting method, instruments may be made for astronomical purposes, racked, and divided on their own centre, and if carefully done would border on perfection itself, consequently I consider it to be the greatest improvement ever made in the art of dividing. I call it self-correcting, because every time it is reversed in cutting the teeth, the screw has a fresh opportunity to correct errours insensible to the eye.

I have well considered the subject, and think, that a circle of twelve inches diameter, made on this principle, would measure angles equally, if not more accurately, than astronomical instruments divided by engines, or by any other methods hitherto used by instruments of any size. It is, therefore, my opinion, that the supposed necessity of making very large circles, for the sake of obtaining correct divisions, will be done away.

JAMES ALLAN.

CERTIFICATES.

Certificates of the utility of the improvement.

After a close consideration of Mr. Allan's improvement in dividing engines, (I mean his mode of racking the teeth only) when combined with the methods at this time known and practised, I look upon it as an important discovery; it is a plan, that in my opinion will admit of a great degree of accuracy, approaching nearly to perfection itself, particularly in circles of small radius, but not quite so applicable in large machines for the purpose of dividing.

JOHN STANCLIFFE.

Little Mary-le-Bone Street, Dec. 15, 1809.

SIR,

The method you have taken to produce a perfect equal racking, for the constructing an accurate dividing engine, is the greatest advance toward perfection that has been communicated to the public within my knowledge; and I believe it to be a method never before practised in this country. It is applicable to the construction of machines of any dimensions, that mathematical or nautical instruments can be graduated by.

Machines now in use less perfect than supposed.

It is my belief, that the greater number of the machines now in use are far short of the perfection they are reputed to have.

I am, Sir,
Your humble servant,
M. BERGE.

Piccadilly, Jan. 8, 1810.
Mr. J. ALLAN.

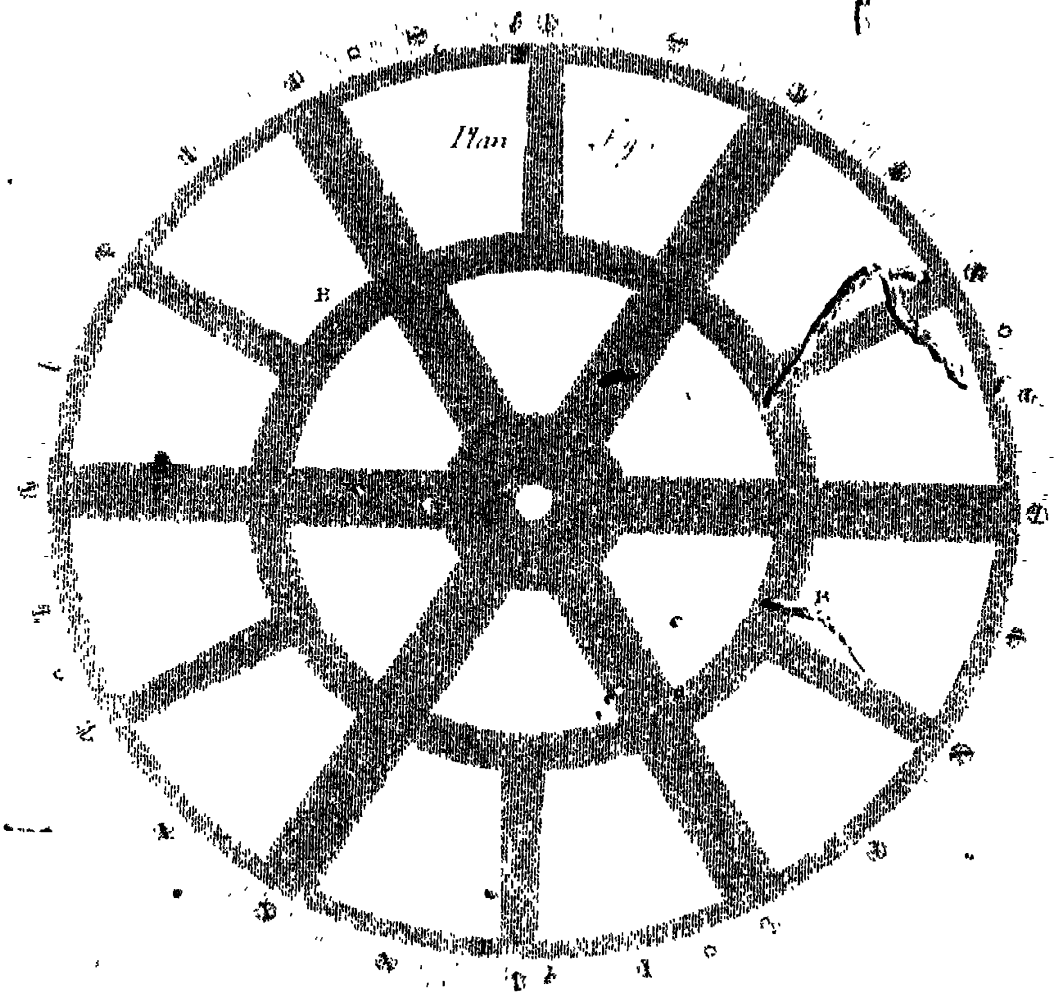
Reference to the Drawing of Mr. Allan's improvement on the Dividing Engine of Ramsden, Pl. II.

Nature of the improvement.

The dividing engine invented by Mr. Jesse Ramsden, and for which he received the reward of the Board of Longitude, in the year 1775, is minutely explained in a quarto pamphlet, published by order of the Commissioners of Longitude; also, in the article *engine*, in Dr. Rees's New Cyclopædia, as well as some other works of a similar nature; it therefore becomes unnecessary for the Society to give any more of Mr. Allan's engine in their drawings than is explanatory of the improvement, the engine being used in the same manner as Ramsden's; this part is the great circle, upon which the arch to be divided is placed, and the circle turned about a determinate quantity at each division, by means of a screw, the threads of which engage fine teeth, cut around the periphery of the circle. The improvement by Mr. Allan consists in the method of cutting or racking these teeth, to ensure their being perfectly of equal size, in all parts of the circle.

Description of the plate.

The plan, fig. 1, in plate II, represents the upper surface of a belmetal circle mounted upon an axis, A, fig. 2, and its surface made truly plane, and perpendicular to the axis; the section shows the figure of the axis, and the central ring B, to give the greatest strength to the circle; C is a section of a portion of the frame of the engine; and D a socket into which the axis A is fitted; the circumference of the large circle is turned to such a figure as to receive a ring of brass, *a*, fig. 3, which is united firmly to it by a number of pins, one of which is shown in the figure. Upon this ring a second, *b*, is placed, the two making the same thickness as the circle. The inside of the ring *b*, and the outside of the belmetal circle, are fitted to each other with the utmost accuracy, and great care taken to turn the said fitting truly concentric with the axis of the circle; the brass rings *a* and *b* are held together by twenty four screws, as shown in the plan; and a groove, corresponding to the curvature of the screw which moves the circle, is turned in the outside of the two; in this state the racking of the teeth is performed by a screw similar to that afterward used to turn the circle to its divisions, but notched across the threads, so that it cuts like a saw, when pressed against the circle and turned round, and removes the metal from the spaces between the teeth, which are by this means formed around the edge of the circle; when this has been performed all round, two fine lines are drawn across the brass and belmetal circles, diametrically opposite



Mr. Flinn's Mathematical Dividing Engine.

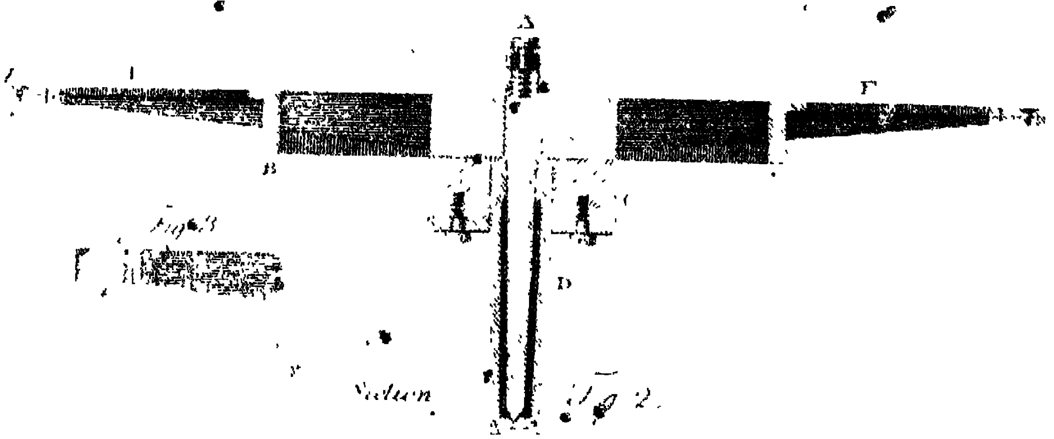


Fig. 3

Section. Fig. 2.

opposite each other; the twenty four screws are then withdrawn, and the upper brass ring turned exactly half round, which is determined by the lines before mentioned; and by this means the teeth of the circle are divided into two thicknesses, and being put together again in opposite directions, if any error arose in racking the teeth, it would be shown by the upper and lower halves of the teeth not coinciding when reversed, and by racking them while reversed the screw would cut away the inequalities, and make all the teeth of the same size and distance from each other; this reversing the teeth is performed several times, till the teeth are brought to a perfect equality in all parts of the circle; four steady pins are accurately fitted into the two rings to hold them together in any of the positions in which they have been racked together, and it is upon these the dependence is placed for the coincidence of the teeth, the twenty four screws being merely to hold them fast together, and fitted rather loosely in their holes, that they may not strain the steady pins.

III.

Observations on the Waste, that Pulverization occasions in Substances by Mr. Henry, *Chief of the central Pharmacy of civil Hospitals, &c**.

THE School of Pharmacy being consulted by count de Cessac, *ministre directeur de l'administration de la guerre*, respecting the waste, that certain mineral, vegetable, and animal substances, employed as medicines, experience on being powdered, appointed two of its members, to examine particularly into a subject so interesting in pharmacy, and to make a report on it.

Inquiry into the waste of drugs by the French Government.

Report of the Committee to the Members of the School of Pharmacy.

Gentlemen,

You have appointed two of your members, to make a report

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* Ann. de Chim. vol. LXXV, p. 264

port to you on the question proposed by count de Cessac; and to ascertain the waste, which pulverization occasions in certain drugs.

Difficulties in it.

This question, which appears at first sight easy to answer, presents notwithstanding some difficulties to the operator, on account of different circumstances, which do not always occur together, and which may consequently alter the results.

1. The greater or less dryness of the substances to be powdered.

2. The quality of these substances.

3. The preparation they are to undergo in passing through the hands of the apothecary.

4. Lastly, the modes of pulverization, and the fineness of the powder.

Causes of waste.

It would be superfluous to point out to you, gentlemen, the rules, that pharmacologists have given for reducing to powder this or that substance: and you know better than any persons, that pulverization requires the substances subjected to it to be very dry, and that they cannot be brought to a proper state of dryness for this operation, without being exposed to the heat of the sun, or of fire. You well know, that the quality is not indifferent with respect to the waste; that all the parts of a vegetable, for instance, should not enter into the preparation of the powder; that roots containing a woody meditullium, barks covered with lichen, and fibrous or downy leaves, produce a bulky, inert residuum, of no use to the apothecary.

These of two kinds.

The waste occasioned by pulverization therefore may be considered as of two kinds. Waste from the preparation of the substance, its division, and desiccation by the fire. Waste from the pounding and the residuum.

Less on a large scale.

An important observation for the operator is, that the waste is less when a hundred weight of any substance is powdered in immediate succession, than when it is powdered in separate parcels of six or ten kilogrammes [14 or 20 lbs.]

In large concerns, by setting aside the residues that may be of use, as those of jalap, cinchona, rhubarb, cinnamon, &c., and using them on subsequent occasions, the waste is less considerable.

All plants reduced to powder, and kept either in bottles or boxes, gain weight by their hygrometric property.

Powders of plants gain weight by absorbing moisture.

To find exactly the loss occasioned by pulverization, we took a metrical hundred weight of each of the substances mentioned below, cleaned, prepared, and in the driest state; and we reduced each to an impalpable powder. The following are the results of the waste of 100 kil. [219 lbs] of each.

Table of waste of certain drugs.

Substances	Produce	Waste
Ipecacuanha	87	13
Jalap	92	8
Rhubarb	93·8	6·2
Squill	87·5	12·5
Cinchona	93·7	6·3
Gum arabic	93·5	6·5
Scammony	95	5
Cantharides	92·7	7·3
Sal ammoniac	98	2
Cream of tartar	97	3
Antimony	97	3
Gum tragacanth	93·6	6·4
Cinnamon	93·6	6·4

IV.

Analysis of some ancient Alloys in the Church of Goslar: by Mr. Klaproth*.

Analysis of the Altar of Krodo.

AMONG the antiquities of the north of Germany, one that had been preserved for some centuries in the church of St. Simon, at Goslar, and was known by the name of the altar of Krodo, but which is now removed to Paris, deserved more attention than had been paid it.

Altar of Krodo.

The legend of this Krodo says, that he was an idol of the pagan Saxons, and had his seat at Hartzburg, on the

Legend of this idol.

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* Ann. de Chim. vol. LXXV, p 317.

Hartz. He was represented as an old man, with a long beard and hideous countenance, standing barefoot on a fish rough with scales and spines, holding a wheel in his left hand, and a basket filled with flowers and fruit in his right.

On this altar the first-born of mothers was sacrificed to him, as to Moloch.

The abolition of the pagan [illegible] by Charlemagne overturned this idol, but his altar was preserved for the use of the Christian church.

Probably fabulous.

The critical history of Germany does not acknowledge any god Krodo, but takes the whole for a fable invented by the monks of the middle age.

However this may be, the altar appears to have been used for burning animals to some deity.

The altar described.

The shape of the altar is a hollow parallelopipedon, three feet three inches long, two feet and a half wide, and two feet seven inches high. It stands on four feet, supported by little men of hideous aspect. It is covered with a slab of white marble.

The metal.

The metal of this altar is of a brass yellow colour, a hackly fracture, and easily polished. Its specific gravity is 8·767.

Analysis of it.

On 200 grains of it nitric acid was poured, which dissolved it completely, without the assistance of heat.

The solution was divided into two parts. Into one of these was dropped a solution of sulphate of soda. The precipitate, when well washed and calcined, presented 18·25 grs of sulphate of lead, answering to 13 grs of metallic lead.

The supernatant liquid was mixed with 200 grs of sulphuric acid, and evaporated to dryness. The mass was redissolved, and iron added, which precipitated 69 grains of copper.

The other half of the solution was mixed with five parts of distilled vinegar, and the mixture poured on a thin plate of hammered lead in a shallow dish.

After a few days, and by the assistance of heat, the copper was precipitated. The liquor being filtered off, the lead was thrown down by sulphate of soda. The supernatant liquid was then precipitated by carbonate of soda and the carbonate of zinc, well washed and calcined, left 22·25 grs of oxide of zinc, answering to 18 grs of metallic zinc.

The alloy of the altar therefore consists of

Copper	69
Zinc	18
Lead	13
	100

Analysis of the alloy of the imperial seat.

The kaiser stuhl

In the church of Goslar there is an arm chair, called the *kaiserstuhl*.

The colour of the metal is a pale copper red: its fracture is porous: its spec. grav. is 8·037.

Analysis of it.

200 grains of the alloy were treated with nitric acid. 12·25 grs of oxide of tin were left. From the decanted liquor, sulphate of soda threw down 7·5 grs of sulphate of lead. From the remaining liquid, after the addition of sulphuric acid in excess, iron separated 165 grs of copper.

This therefore is composed of

Copper	92·5
Tin	5
Lead	2·5
	100.

Analysis of a large chandelier.

Analysis of an antique chandelier.

200 grs were treated by nitric acid, and the solution divided into two parts.

In one of these sulphate of soda occasioned no precipitate: but iron separated from it 84 grs of copper.

The other half was treated as in the first analysis, and as much oxide of zinc was obtained, as answered to 16 grs of the metal.

Consequently this alloy consists of

Copper	84
Zinc	16
	100.

IV.

An Account of a Vegetable Wax from Brazil. By WILLIAM THOMAS BRANDE, *Esq., F. R. S.**

Vegetable wax from Brazil.

SECT. I. THE vegetable wax, described in this paper was given to the president by Lord Grenville, with a wish, on the part of his Lordship, that its properties should be investigated, in the hope that it might prove a useful substitute for bees wax, and constitute, in due time, a new article of commerce between the Brazils and this country.

Tree whence it is obtained.

It was transmitted to lord Grenville from Rio de Janeiro, by the comte de Galveas, as a new article lately brought to that city, from the southernmost parts of the Brazilian dominions, the capitanias of Rio Grande and Seara, between the latitudes of three and seven degrees north. It is said to be the production of a tree of slow growth, called by the natives *carnauba,* which also produces a gum used as food for men, and another substance employed for fattening poultry.

When the comte wrote to Lord Grenville in July last, orders had been sent to the governors of the districts where it grows, requiring them to report more particularly on the nature and qualities of this interesting tree; we may therefore hope, that information will soon be obtained, whether the article can be procured in abundance, and at a reasonable price, in which case it will become a valuable addition to the comforts of mankind, by reducing the price and improving the quality of candles, flambeaux, &c.

[illegible] ceroxylon andicola.

The article, in the state in which it was sent, resembles much that described by Humboldt, as the produce of the cola of Humboldt.

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* Phil. Trans. for 1811, p. 261.

the ceroxylon andicola*; but it is not likely to be the same, as Humboldt's wax is collected from a stately palm tree, which grows on the high mountains, from 900 to 1450 toises above the level of the sea, and on the edge of the regions of perpetual snow. On the other hand, the Brazilian plant is described as a slow growing tree, but not as a large one, and there are no hign mountains delineated in the most accurate and recent maps of the capiteneas where it is found. But a more decisive argument against their identity is the analysis of Vauquelin, published by Humboldt, which shows, that the produce of the ceroxylon consists of two thirds resin and only one third wax; but the Brazilian article is entirely wax, and affords not the smallest trace of resin. The Brazilian plant, however, was not entirely unknown to Humboldt, for it appears from his book, that Mr. Correa had informed him, that a palm, called *carnauba* by the natives of Brazil, produced wax from its leaves.

The wax described.

SECT. II. 1. The wax, in its rough state, is in the form of a coarse pale gray powder, soft to the touch, and mixed with various impurities, consisting chiefly of fibres of the bark of the tree, which, when separated by a sieve, amount to about 40 per cent.

It has an agreeable odour, somewhat resembling new hay, but scarcely any taste.

At 206° Fahrenheit it enters into perfect fusion, and in this state it may be farther purified, by passing it through fine linen. By this process, it acquires a dirty green colour, and its peculiar smell becomes more evident. When cold, it is moderately hard and brittle. Its specific gravity is 0·980.

Insoluble in water,

2. Water exerts no action on the wax, unless boiled with it for some hours; it then acquires a slight brown tinge, and the peculiar odour of the wax.

and in alcohol unless heated.

3. Alcohol does not dissolve any portion of the wax, unless heat be applied.

Two fluid ounces of boiling alcohol, spec. grav. 0·826, dissolve about ten grains of the wax, of which eight grains are deposited as the solution cools, and the remaining two grains may be afterward precipitated by the addition of water, or may be obtained unaltered by evaporating the alcohol.

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* Plantes equinoctiales, p

The solution of the wax in alcohol has a slightly green tinge.

4. Sulphuric ether, spec. grav. 0·7563, dissolves a very minute portion of the wax, at the temperature of 60°.

Two fluid ounces of boiling sulphuric ether dissolve thirty grains of the wax, of which twenty-six grains are deposited by cooling the solution, and the remaining four grains may be obtained by allowing the ether to evaporate spontaneously.

5. The fixed oils very readily dissolve the wax at the temperature of boiling water, and it forms with it compounds of an intermediate consistence, very analogous to those which are obtained with common bees wax.

In examining some combinations which I had made of the vegetable wax with olive oil, I was surprised to find them perfectly soluble in ether, and sparingly soluble in boiling alcohol.

As it is commonly stated, that the fixed oils are insoluble in ether and in alcohol, I was led to attribute the solution of the oil, in these instances, to its being combined with the wax, but subsequent experiments, of which I shall state the general results, have shown me, that the opinion is erroneous.

Four fluid ounces of sulphuric ether, spec. grav. [illegible], dissolve a fluid ounce and a quarter of the expressed oil of almonds; of olive oil, the same quantity of the ether dissolves a fluid ounce and a half; of linseed oil, two fluid ounces and a half, and castor oil is soluble in any proportion in sulphuric ether of the above specific gravity.

The expressed oils of almonds and of olives are very sparingly soluble in alcohol, spec. grav. 0·820.

Linseed oil is more soluble than the two former. Four fluid ounces of alcohol, spec. grav. 0·820, dissolve nearly one fluid drachm.

Castor oil is perfectly soluble in every proportion in alcohol, spec. grav. 0·820. In alcohol of a higher specific gravity, as 0·840, it is very sparingly soluble*.

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* The solubility of castor oil in alcohol was mentioned to me some months ago by Dr. Wollaston, who also informed me, that it had on this account been employed to adulterate certain essential oils of high value, [illegible] the oil of cloves.

Effect of camphor.

As some of the difficultly soluble resins are more easily dissolved in alcohol, to which a small proportion of camphor has been added, I endeavoured to ascertain, whether the fixed oils were rendered more soluble by the same means, but found, that this was not the case, excepting with regard to castor oil, which, although very sparingly dissolved by alcohol of a spec. grav. above 0·840°, becomes abundantly soluble, by the addition of one part of camphor, to eight parts of the alcohol.

Heat increases the solvent power of alcohol

Boiling alcohol, spec. grav. 0·840, takes up a considerable portion of castor oil and of linseed oil; it also dissolves a small quantity of the oils of almonds and of olives; but they are copiously deposited during the cooling of the alcohol, and only a small portion retained in permanent solution.

Water precipitates solutions of the oils.

When water is added to any of these solutions of the fixed oils in ether, and in alcohol, a milky mixture is formed, and the oil gradually separates upon the surface, without having undergone any apparent alteration.

Action of potash on the wax:

6. One hundred grains of the wax were boiled for half an hour in a solution of caustic potash, spec. grav. 1·090. The solution acquired a pale rose colour, but appeared to exert no farther action on the wax, which, after having been washed with warm water, retained its fusibility and other properties. No combination therefore, similar to a soap, was produced; nor was any precipitate occasioned by the addition of acids to the rose coloured alkaline solution.

of soda and mild alkalis

7. The effects produced by boiling the wax in solutions of pure soda, and of the subcarbonates of soda and of potash, were analogous to those of the caustic potash.

of ammonia:

8. Solutions of pure and of carbonated ammonia exert scarcely any action on the wax.

of nitric acid.

9. When the wax is boiled in nitric acid, spec. grav. 1·45. there is some escape of nitrous gas, and the colour of the wax is gradually changed to a deep yellow.

When the wax is removed from the acid, and washed with hot water, it is found to have become more brittle and hard, but it still retains much of its peculiar odour.

In this state it remains insoluble in the alkalis, but they now change its colour to a very bright brown, which is destroyed by washing with dilute muriatic acid, and its original yellow colour restored.

Neither the fusibility, nor the inflammability of the wax, is impaired by this process.

Nitric acid, diluted with eight parts of water, produces the same change in the colour of the wax as the concentrated acid.

Attempts to bleach the wax.

Having been unsuccessful in my attempts to bleach the wax in its original state, I made some experiments to ascertain whether its colour could be more easily destroyed, after it had been acted upon by nitric acid; and found, that, by exposing it spread upon glass to the action of light, it became in the course of three weeks of a pale straw colour, and on the surface nearly white. The same change was produced, by steeping the wax, in thin plates, in an aqueous solution of oximuriatic gas, but I have not hitherto succeeded in rendering it perfectly white.

Action of muriatic acid:

10. Muriatic acid has little action on the wax: when boiled upon it for some hours, it destroys much of its colour.

of sulphuric:

11. Sulphuric acid changes the colour of the wax to a pale brown, and when water is added, it becomes of a deep rose colour; the inflammability and the fusibility of the wax are slightly impaired by this process.

When heat is applied, the wax is decomposed with the usual phenomena, sulphurous acid is developed, and charcoal deposited.

of acetic:

12. Acetic acid has very little action on the wax, when cold.

When the wax is boiled in this acid, a minute portion is dissolved, and again deposited as the solution cools. By long continued boiling in acetic acid, the wax is rendered nearly white; but when it is afterward washed with water, and fused, it resumes its former colour.

and of oximuriatic gas.

13. When the wax is fused in oximuriatic gas, it is rapidly decomposed, and, parting with hidrogen and oxigen, muriatic acid and water are formed, and charcoal is deposited.

Products of distillation.

14. The results of the destructive distillation of the vegetable wax are very analogous to those of bees wax.

An acid liquor, mixed with a volatile oil, are the first products; these are succeeded by a large proportion of a butyraceous oil; and a very small quantity of charcoal, affording traces of lime, remains in the retort. During the process, a little carburetted hidrogen gas is given off.

I have not considered it necessary to dwell upon the relative proportions of these different products, as they will necessarily vary according to the rapidity, with which the distillation is conducted.

This wax differs from other varieties.

SECT. [illegible] From the preceding detail of experiments, it appears, that, although the South American vegetable wax possesses the characteristic properties of bees wax, it differs from that substance in many of its chemical habitudes; it also differs from the other varieties of wax, namely, the wax of the myrica cerifera[*], of lac[†], and of white lac[‡].

Bleaching.

The attempts, which I have made to bleach the wax, have been conducted on a small scale; but from the experiments related, it appears, that, after the colour has been changed by the action of very dilute nitric acid, it may be rendered nearly white by the usual means. I have not had sufficient time to acertain, whether the wax can be more effectually bleached by long continued exposure, nor have I had an opportunity of submitting it to the processes employed by the bleachers of bees wax.

Perhaps the most important part of the present inquiry is that, which relates to the combustion of the vegetable wax, in the form of candles.

Its use in candles.

The trials which have been made, to ascertain its fitness for this purpose, are extremely satisfactory; and, when the wick is properly proportioned to the size of the candle, the combustion is as perfect and uniform, as that of common bees wax.

The addition of from one eighth to one tenth part of tallow is sufficient, to obviate the brittleness of the wax in its pure state, without giving it any unpleasant smell, or materially impairing the brilliancy of its flame. A mixture of three parts of the vegetable wax, with one part of bees wax, also makes very excellent candles.

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[*] Vide Dr. Bostock's Experiments on the Wax of the Myrica cerifera, in Nicholson's Journal for March, 1803; vol. IV, p. 130.

[†] Vide Analytical Experiments and Observations on Lac, by Charles Hatchett, Esq. F. R. S., in the Philosphical Transactions for 1804; or Journal, vol. X, p. 45, 95.

[‡] Vide Observations and Experiments on a Waxlike Substance from Madras, by George Pearson, M. D. F. R. S., in the Philosophical Transactions for 1794.

VI.

Observations on the Alkaline Oxalates and Superoxalates, and particularly on the Proportions of their Elements: by Mr. J. E. Berard*.

Dr. Thomson on oxalic acid and the oxalates.

DR. Thomson has just published in the Philosophical Transactions† a very interesting paper on oxalic acid. One part of it is dedicated to the determination of the proportions of the oxalates, and for this he has taken the following method.

After having carefully ascertained the proportions in oxalate of lime, by combining a known quantity of lime with oxalic acid, he could find the quantity of real acid contained in a given weight of a solution of oxalic acid.

He then took 100 grains of a solution of oxalic acid, containing 7 grains of real acid; and, neutralizing it successively by the different alkalis, he ascertained the quantity of oxalate produced.

He did not examine the superoxalates,

But it is well known, that this acid has the property of forming with some bases salts with excess of acid: and, as the method of Dr. Thomson could not make known their proportions, he did not examine these. In the next place, I observed in his table some proportions, which could not agree with the capacities of the alkalis for saturation hitherto observed.

and some of his proportions doubted.

The subject therefore revised.

These considerations induced me, to repeat the analyses of the oxalates, and to examine particularly the superoxalates. I also took a method different from Dr. Thomson's, when it was necessary and practicable: because, if I obtained the same results in another mode, this would increase our confidence in them.

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* Ann. de Chim. vol. LXXIII, p. 263. Read to the Institute, Jan. the 29th, 1810.

† Phil. Trans. 1808, p. 63: or Journ vol. XXI, p. 14, 86.

Oxalate

Oxalate of Lime.

Oxalate of lime examined.

The proportions of oxalate of lime being to serve as a basis for all my analyses, I neglected nothing, to ascertain them with precision.

As we can employ only a gentle heat to dry this salt, we can never be sure of having it entirely free from water. This is a slight source of uncertainty, which the means of chemistry have not yet enabled us to remove.

Having obtained this salt very pure, by precipitating muriate of lime with oxalate of ammonia, I dried it at the heat of boiling water, till I could no longer discover any diminution of its weight. This oxalate of lime I considered as dry. Ten gram. of this, exposed to a violent heat, left 3·8 of lime, that caused no effervescence with acids. This was the mean of four experiments. Accordingly I fixed the proportions of oxalate of lime at

Its component parts.

'62 oxalic acid,
38 lime.

100.

The proportions found by Dr. Thompson[*] are very near these.

No superoxalate of lime.

Oxalate of lime is almost wholly insoluble in an excess of its acid; whence we may infer, that there is no superoxalate of lime.

The following experiment seems to prove, that the oxalate of lime, as analysed by me, may be considered as very nearly free from water, without being liable to any great errour.

Oxalic acid sublimed.

In a glass retort I distilled some crystals of very pure oxalic acid. All the phenomena already described by Bergman[†] presented themselves. The crystals liquefied, and some oxalic acid was carried over into the receiver by the water of crystallization; but the matter in the retort soon became solid. A small quantity of oxicarburetted hidrogen gas was then evolved; and abundance of white vapours arose, tolerably dense, and very acrid. At the same time the top of the retort became lined with fine acicular crystals of oxalic acid.

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[*] 62·5 acid, 37·5 base.

[†] Opuscula.

In this state the acid is very light, very white, and a little attractive of the moisture of the atmosphere. If put into water, it becomes pasty before it dissolves.

Oxalate of lime compounded of this.

From my experiments it appears, that this sublimed acid is as dry as that which exists in the oxalate of lime. I weighed 3·42 gram., dissolved them in water, neutralized them with ammonia, precipitated with muriate of lime, and carefully washed the precipitate. To collect and weigh it, I threw it on a filter, which was placed on another filter of the same weight. I then dried them gently, till I could easily separate the precipitate, which I afterward exposed to the heat of a water bath. It weighed 5·374 gr.: but the weight of the inner filter exceeded that of the outer by 0·086 of a gr.; ſo that I had 5·46 gr. of oxalate of lime, which, according to the analysis I have given above, contained 3·385 gr. of acid, being nearly equal to the quantity of sublimed acid I employed*.

A second experiment confirmed these results.

Its insolubility examined.

Before I undertook the subsequent experiments, I satisfied myself, that the oxalate of lime was sufficiently insoluble, to indicate with precision the quantity of acid contained in a compound. 5 gr. [77 gr.] of lime-water neutralized by muriatic acid were diluted with 400 gr. of distilled water; and in one twentieth of this oxalate of ammonia produced a precipitate, that could be perceived without hesitation. This was more than sufficient for analyses of such a nature.

But the least excess of acid dissolves a large quantity of this salt; hence I have always taken care to employ it with neutral compounds.

Crystallized oxalic acid.

Quantity of real acid in crystals of oxalic acid.

To repeat the experiments of Dr. Thomson, crystallized oxalic acid always appeared to me more commodious than a solution of this acid: and in order to ascertain the quantity of real acid contained in what I was to employ, I neutralized

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* According to the proportions assigned by Dr. Thompson, it contained 3·412, which comes very near indeed to the quantity used. C.

10 gr. with ammonia, and precipitated by muriate of lime. Thus I obtained 11·73 gr. of oxalate of lime; so that the acid analysed contained

72·7 of real acid,
27·3 water.
100.

As this was to be employed in all my experiments, in order to have it in a uniform state it was powdered, and kept in a well stopped phial.

Oxalate of potash.

Oxalate of potash.

This salt is so soluble in water, that it is very difficult to crystallize it.

Decomposed by fire, and

10 gr. were urged in the fire in a crucible, and 6 gr. of fused carbonate of potash, insoluble in alcohol, were obtained. Now I have observed, that all the carbonates possessing these two properties are uniform in their proportions*; and I have settled the proportion of potash to carbonic acid in this salt to be as 100 to 42·42: the subcarbonate resulting from this experiment therefore contained 4·212 gr. of potash.

by muriate of lime.

10 gr. of the same oxalate, precipitated by muriate of lime, yielded 6·543 of oxalate of lime.

Its component parts.

The elements of the salt I analysed, therefore, were

42·12 potash,
40·57 oxalic acid,
17·31 water.
100.

Another experiment.

In another experiment, 10 gr. of the oxalic acid, analysed above, were accurately neutralized with caustic potash; the oxalate was evaporated to dryness, and exposed to a strong heat in a platina crucible; and 10·96 gr. of fused subcarbonate of potash were the result.

Proportion of the dry oxalate.

Taking a mean between these two results, I fix the proportions of dry oxalate of potash at

50·68 potash,
49·32 oxalic acid.
100.

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* Ann. de Chim. vol. LXXI, p. 50.

Hence it follows, that 100 of potash combine with 97·3 of oxalic acid.

Superoxalate of potash.

Superoxalate of potash, or salt of sorrel.

This salt I obtained from a solution of the neutral salt, to which I had added an excess of acid. This superoxalate is known in the shops by the name of salt of sorrel. It is less soluble than the neutral oxalate.

10 gr. urged in the fire yielded 4·91 of fused carbonate of potash=3·46 of potash.

10 gr. of the same salt, neutralized by ammonia, and precipitated by muriate of lime, yielded 10·6 of oxalate of lime=6·58 of oxalic acid.

Its component parts.

This gives for the composition of superoxalate of potash

65·8 oxalic acid,
34·2 potash.

100.

Consequently 100 of potash combined with 192·4 of oxalic acid.

Dr. Wollaston's quadroxalate of potash.

Method of obtaining quadroxalate of potash.

This may be obtained in several ways; either by adding acid to the superoxalate; or by causing the muriatic, sulphuric, or nitric acid to act on the superoxalate; or by boiling crystals of oxalic acid in a solution of muriate of potash. What determines the separation of the quadroxalate is its being less soluble than either of the two compounds just examined.

After having purified the salt, obtained by one of these means, by a second crystallization, I dried it on a water-bath, and subjected it to the same analysis.

10 gr. urged in the fire yielded 2·7 of subcarbonate of potash=1·895 of potash.

10 gr., brought to the neutral state by ammonia, and precipitated by muriate of lime, yielded 11·62 gr. of oxalate of lime=7·205 gr. of oxalic acid.

Its component parts.

100 gr. of quadroxalate of potash, therefore, are composed

18·95 of potash,
72·05 of oxalic acid,
9 of water.

100.

Hence it follows, that 100 parts of potash are combined with 381 of oxalic acid in this salt. And from the preceding analyses, compared with this, that 100 parts of potash are combined with

Proportions of acid in the three salts.

97·6 of acid, in the neutral oxalate,
192 ——— in the superoxalate, and
381 ——— in the quadroxalate.

These quantities are to each other nearly in the ratio of the numbers 1, 2, 4.

For the knowledge of this curious fact we are indebted to Dr. Wollaston[*]. I have repeated his experiments, and confirmed their results.

Potash will not combine with more acid.

I endeavoured by other means, to combine potash with a larger quantity of acid, but found I could not. Having evaporated a solution of quadroxalate of potash, to which I had added a very large quantity of oxalic acid, the first crystallization separated the quadroxalate; and I could obtain nothing afterward but crystals of oxalic acid free from potash[†].

Oxalate of soda.

Oxalate of soda.

This is very sparingly soluble in water; in which respect it differs much from the oxalate of potash, which on the contrary dissolves very easily in this fluid.

10 gr. of crystallized oxalic acid were dissolved in water, and neutralized by soda. The oxalate was evaporated to dryness, and heated strongly in a platina crucible. The result was 8·1 gr. of subcarbonate, containing 5·064 of soda. Hence 100 parts of oxalate of soda consist of

Its component parts.

58·92 oxalic acid,
41·08 soda.
———
100.

And 100 of soda combine with 143·5 of oxalic acid.

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[*] Bibliothèque Britannique. [Philos. Trans., for 1807, p. 95: or Journal, vol. XXI, p. 164.]

Salt of sorrel sometimes a quadroxalate.

[†] When I began my experiments, I had procured some salt of sorrel from a very respectable druggist; and I attempted to combine it with a larger quantity of acid by the methods indicated by Dr. Wollaston, but without success. Hence I was about to conclude, that the quadroxalate did not exist; when I discovered, that the salt on which I was operating was a real quadroxalate. This proves, that the salt of sorrel of the shops is sometimes a quadroxalate, and consequently combined with too much acid.

Superoxalate of soda.

Superoxalate of soda.

The superoxalate of soda is less soluble than the neutral oxalate. It may be obtained by direct combination of the oxalic acid with soda, or by the action of oxalic acid on muriate of soda.

10 gr. of this salt, urged in the fire, yielded 4·09 gr. of subcarbonate of soda = 2·557 gr. of soda.

10 gr. of the same salt, precipitated by muriate of lime, yielded 11·741 of oxalate of lime = 7·28 gr. of oxalic acid. The proportions of this salt therefore are

Its component parts.

25·57	soda,
72·80	oxalic acid,
1·63	water.
100.	

And 100 parts of soda are combined in it with 284·7 of acid.

Its acid double that of the oxalate.

It appears by the proportions I have given, that in the acid oxalate of soda the base is combined with twice as much acid as in the neutral oxalate, analogous to what occurs with potash. In confirmation of this, I urged in the fire 10 gr. of superoxalate of soda; and the alkali, resulting from its decomposition, was sufficient exactly to neutralize 10 gr. of the same superoxalate.

No quadroxalate.

I tried in several ways to combine soda with a larger quantity of acid, but I could not succeed; so that I believe no quadroxalate of soda exists.

Oxalate of ammonia.

Oxalate of ammonia.

Mr. Berthollet has ascertained by very accurate experiments, that a solution of ammonia of the specific gravity of 0·9656 contains 8·761 of real ammonia in 100. Adopting this datum, the most convenient method of determining the proportions of the compound of ammonia with oxalic acid appeared to me to be, to find the quantity of this ammonia necessary to neutralize a given weight of oxalic acid.

5 gr. of crystallized oxalic acid required for their neutralization 9·5 of ammonia of the spec. grav. abovementioned,

ed, or 0·83 of real ammonia. We have therefore for the proportions of dry oxalate of ammonia

Its component parts.

27·66 ammonia,
62·34 oxalic acid*.

Consequently 100 of ammonia combine with 261·4 oxalic acid.

Water in the crystals.

I ascertained the quantity of water contained in crystallized oxalate of ammonia by precipitating it with muriate of lime, and found it was 13 in 100.

I likewise employed neutralization by ammonia to find the excess of acid in the superoxalates: and I found the results analogous to those I have already given.

Thus, after having found by calcination, that 10 gr. of superoxalate of potash contained 3·46 of potash; I found, that it required 1·254 of real ammonia, to bring them to the neutral state. The quantity of acid of the superoxalate of potash therefore is 3·32 gr. neutralized by the potash in it, added to 3·28 neutralized by the ammonia. This result, extremely near what I have given, proves, that the excess of acid of the superoxalate is equal to that which is neutralized by the potash in the salt.

Superoxalate of ammonia.

Superoxalate of ammonia.

The superoxalate of ammonia too is less soluble than the neutral oxalate.

10 gr. of this salt yielded 11·84 of oxalate of lime = 7·34 of oxalic acid; a quantity capable of neutralizing 2·81 of ammonia. If then the alkali in the superoxalate of ammonia were combined with twice as much acid as in the neutral oxalate, the quantity of ammonia, necessary to bring the salt in question to the neutral state, must be 1·4. Now I found it by experiment to be 1·35.

We

I have given the figures here as they stand in the original; but, as [illegible] was evidently some mistake, the amount being only 90, I endeavoured to find where the errour lay. From what follows, combined with the proportion of real acid before assigned to the crystallized acid by the author, it appears, that the 5 in the beginning of the paragraph should have been a 8; and the proportions, 27·66 ammonia to 72·34 oxalic acid; or 100 ammonia to 261·53 acid. C.

We may therefore consider this position as true; and in this case the superoxalate of ammonia would be composed of

Its component parts.

73·4	oxalic acid,
14	ammonia,
12·6	water.
100.	

Whence it follows, that 100 of ammonia are combined with 523 of oxalic acid in this salt.

I could never succeed in the attempt to form quadroxalate of ammonia.

Oxalate of strontian.

Oxalate of strontian.

This salt is nearly insoluble in water.

10 gr. of crystallized oxalic acid were dissolved in water, and neutralized by strontian. The oxalate of strontian, being evaporated to dryness, and exposed to a strong heat in a platina crucible, left 11·9 gr. of carbonate of strontian*; which, having been decomposed by nitric acid, yielded 8·687 of strontian.

From this analysis we have for the composition of oxalate of strontian

Its component parts.

45·54	oxalic acid,
54·46	strontian.
100.	

Whence it follows, that 100 of strontian unite with 83·62 of acid.

Dr. Thomson deceived in his proportions.

Dr. Thomson has certainly been deceived in his calculation respecting the oxalate of strontian. It would follow, indeed, from the numbers he gives, that barytes has a greater capacity for saturation than strontian, which is contradictory

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Component parts of carbonate of strontian.

* As I have been under the necessity of repeating this experiment several times, I took great care to ascertain the proportions of carbonate of strontian. The mean of all my experiments gave

Strontian	73·6
Carbonic acid	26·4
	100.

tradictory to all the analyses of salts with base of strontian and barytes hitherto known.

Supposed he had formed superoxalate of strontian

On the other hand, after having ascertained, by an experiment similar to that I have just mentioned, the proportions of oxalate of strontian, he repeated it a second time by first neutralizing a given weight of oxalic acid with ammonia, and afterward precipitating by muriate of strontian: but in this way he found, that the strontian combined with twice as much acid as he found at first; whence he inferred, that the salt obtained in this process was a superoxalate of strontian, in which the strontian was combined with twice as much acid as in the neutral oxalate.

but this does not exist.

The little solubility of the oxalate of strontian perhaps misled Dr. Thomson; but it seems to me demonstrated, that, in precipitating neutral muriate of strontian by the neutral oxalate of ammonia, a salt with excess of acid cannot be formed, for the residuum remains neutral. In the next place, I do not think, that an acid oxalate of strontian exists; for I have not been able to form it, in employing the same means as for the other oxalates; and, besides, the neutral oxalate of strontian is very little soluble in an excess of its acid. Lastly, since the proportions he gives for the neutral oxalate are not accurate, his simple ratio between the neutral and acid oxalate is done away.

Oxalate of barytes.

Oxalate of barytes.

This salt is more soluble in water than oxalate of strontian.

10 gr. of the same crystallized oxalic acid were neutralized by barytes. The oxalate, urged in the fire, yielded 15·3 gr. of carbonate of barytes; which, when decomposed by sulphuric acid, left 11·934 gr. of barytes*.

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* I have found by my experiments, that carbonate of barytes is composed of

Component parts of carbonate of barytes

Barytes	78
Acid	22
	100.

These are the same proportions, as Klaproth assigns it

From this analysis the elements of oxalate of barytes must be

Its component parts.

62·17 barytes,
37·83 oxalic acid,

100.

Whence it follows, that 100 of barytes combine with 60·84 of oxalic acid.

Superoxalate of barytes.

Superoxalate of barytes.

When crystals of oxalic acid are boiled in a solution of muriate of barytes, and the liquor is afterward allowed to cool, crystals are deposited, which are superoxalate of barytes. The formation of this salt was first noticed by Darracq*.

Decomposable by water.

This combination has so little stability, that boiling the salt in water is sufficient, to deprive it of all its excess of acid†.

To analyse it, I urged in the fire 10 gr.; and thus found, that they contained 4·504 of barytes.

I also boiled 10 gr. of the same salt in distilled water, which dissolved out all its [excess of] acid; and 1·102 of real ammonia were necessary, to neutralize the liquid. The acid contained in this salt therefore was

Its component parts.

2·74 saturated by the barytes in the salt,
2·80 saturated by ammonia.

5·50

Superoxalate of barytes, therefore, is composed of

55 oxalic acid
45 barytes.

100.

And 100 of barytes combine with 123 of oxalic acid.

Twice as much acid as the neutral salt.

Thus we see too, that barytes is combined with twice as much acid in the superoxalate, as in the neutral oxalate.

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* Ann. de Chim., vol. XL, p. 69.

† Thomson's System of Chemistry vol. IV

Oxalate of magnesia.

Oxalate of magnesia.

This salt is completely similar to oxalate of lime in many respects. I analysed it in the same manner, because the little solubility of magnesia did not admit of neutralizing a given weight of the acid by this alkali.

10 gr. of this salt, dried on a water-bath till its weight was no longer diminished by the heat, were exposed to a strong heat in a platina crucible, and yielded 2·86 gr. of magnesia, containing 0·125 of a gr. of carbonic acid.

We have therefore for the proportions of oxalate of magnesia

Its component parts.

27·35	magnesia,
72·65	oxalic acid.
100.	

This gives 265·6 of oxalic acid to 100 of magnesia.

Sulphate of magnesia not immediately precipitated by oxalate of ammonia, but slowly decomposed.

The oxalate of magnesia is extremely little soluble in water, and in an excess of its acid. Yet, when a solution of sulphate of magnesia is mixed with one of oxalate of ammonia, no precipitate is produced. Dr. Thomson, in relating this fact, seems to oppose it to the principle, that the separation of salts is determined by the force of cohesion: but I have observed, that letting the mixture stand some time is sufficient, to precipitate the oxalate of magnesia completely, without our being capable of redissolving it.

Comparison of the results.

Such are the proportions that result from my analyses of the oxalates. Some of them differ from those, which Dr. Thomson has given*; so that it was not till I had repeated them with all the care, of which I was capable, that I placed confidence in my results. What appears to me to confirm them is, that they agree much better with the capacities of the alkalis for saturating acids already admitted.

For the sake of a more ready comparison, I shall here give a tabular view of my analyses and those of Dr. Thomson; and I shall add, in the last column, the propor-

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Dr. Thomson used too small quantities.

* I believe, the principal difference between Dr. Thomson's analyses and mine arose from that gentleman, certainly a very expert chemist, having operated with too small quantities.

tions calculated from the capacity of the alkalis for muriatic acid*, supposing, that those of oxalate of lime are accurate.

Tabulated salts.

Names of the neutral salts.	Acid.	Base obtained in my expe-	Base obtained in Dr. Thomson's	Base calculated from the capacity for saturation.
Oxalate of lime	100	61·2	60	61.2
——— of potash	100	102·7	122·86	103·8
——— of soda	100	69·7	57·14	68·9
——— of amm.	100	38·2	84·12	
——— of stron.	100	119·5	151·51	113·4
——— of baryt.	100	164·3	142·86	164·3
——— of magn.	100	37·6	35·71	†

All the oxalates do not combine with more acid.

All the oxalates have not the property of combining with an excess of acid, as my experiments show. It is the force of cohesion of the acid, combined with that of the alkali, which determines the existence of the superoxalates.

The acid has great force of cohesion,

In fact, the great number of insoluble salts, which the oxalic acid forms with the bases, tends to prove, that this acid possesses great force of cohesion. To this quality is owing its property of forming with the soluble alkalis salts with excess of acid less soluble than the neutral salts‡.

which accounts for this,

Accordingly the soluble oxalates alone can take up an excess of acid. It is true the oxalate of barytes, which is but sparingly soluble, is capable of forming a superoxalate; but the excess of acid is so feebly retained in this compound, that the action of water is sufficient to separate it.

and for the formation of quadroxalate of potash.

We may farther consider it as a natural consequence of what I have just observed, that potash, which forms the most soluble superoxalate, is capable of forming a quadroxalate, while on the contrary the little solubility of the superoxalates of soda and ammonia, added to the great capacity for saturation of these bases, prevents them from forming quadroxalates.

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* I have taken the proportions of the muriates ascertained by Mr. Rose, whose accuracy is well known. I omit however the muriate of ammonia, because he analysed the salt obtained by sublimation, in which state it contains a slight excess of acid, and, no doubt a little water.

† The agreement between the proportions in this column and those deduced from my experiments is so striking, that I feel it necessary to declare, that my experiments were finished before the calculations were made.

‡ Statique chimique, tom. I, p. 351.

The conclusions, that may be drawn from the observations I have here submitted to the judgment of the class, are:

General conclusion

1st, That the soluble oxalates alone are capable of taking up an excess of acid, and forming salts less soluble than the neutral salts:

2d, That the property of forming superoxalates depends on the force of cohesion of the acid, combined with that of the alkali[*].

3d, That potash is the only alkali capable of forming a quadroxalate.

4th, That, in all the superoxalates, the alkali is constantly combined with twice as much acid as in the corresponding neutral oxalate.

VII.

Observations on Acetate of Alumine: by Mr. Gay-Lussac[†].

Heat precipitates acetate of alumine, which is redissolved by cooling.

I Long ago remarked, that, when a solution of acetate of alumine is heated, it soon grows turbid, and lets fall a large quantity of alumine. In this there is nothing strange, and it is easily explained: but, if the acetate be allowed to cool, we shall see the precipitate gradually dissolve, and the liquid resume its transparency. If the saline solution be heated a second time, it will become turbid anew, and again transparent on cooling. I have repeated these operations twenty times following, and the results have been constantly the same.

Experiment to show this.

Acetate of alumine made with cold saturated solutions of alum and acetate of lead, and consequently but little concentrated, became turbid at 50° cent. [122° Fahr.]. It being then filtered, and exposed to a somewhat higher temperature, a precipitate was formed again. On cooling it did not resume its transparency immediately below the point at which it lost it; it was only at a much lower temperature, that the alumine was wholly dissolved. This is owing to the coherence the earth has acquired; and it is observable, that, the longer the heat has been continued, or the higher it has been raised, the more difficultly the alumine redissolves.

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[*] By force of cohesion I mean the tendency to form insoluble compounds.

[†] Ann. de Chim. vol. LXXIV, p. 193.

Another acetate of alumine, much more concentrated than the preceding, and which was very acid, because a considerable sediment had formed in it, became likewise turbid by heat, but a little slower; and this equally resumed its transparency on cooling.

Nearly half as much thrown down by heat as by ammonia.

To determine the quantity of alumine precipitated from the acetate by heat, and which varies according to the temperature, I took two equal portions of acetate of alumine obtained by the mixture of two solutions of alum and acetate of lead made without heat. One of these portions was heated to ebullition, and immediately filtered: the other was precipitated by ammonia. Both precipitates having been washed and dried, the weight of the first was found equal to nearly half of the second.

This of importance to calico printers.

These observations may be of great importance to calico-printers; for, to obtain mordants highly concentrated, they employ hot solutions of alum and acetate of lead. Much alumine therefore must be precipitated; and, if the mixture be filtered immediately, there will be a considerable loss. To avoid this, it should be suffered to cool completely, before it is filtered, or decanted off; and frequently stirred, that the alumine may redissolve. Without these precautions the acetate of alumine would be very acid; and this no doubt is the reason, why it is usual to add chalk to it. It is easy however, to prevent the decomposition of the acetate of alumine by heat. The addition of alum, which, as is well known, has the property of dissolving alumine, will for this reason prevent the acetate from becoming turbid. A great excess of acid would answer the same purpose as alum.

Method of preventing it.

From the preceding observations too we may easily conceive the reasons of the copious precipitation, that sometimes takes place in solution of acetate of alumine. The precipitate retains some acid as well as that obtained by the heat of ebullition; for water dissolves a part of it, and sulphuric acid expels acetic acid from it: however, it may be completely removed by repeated washings with hot water.

The precipitation is not owing to volatilization of the acid.

The precipitation of alumine by heat, and its solution at a lower temperature, are facts interesting to the general theory of chemistry, and have very few analogous to them. If this precipitation were owing to the volatilization of acetic acid, the alumine could not redissolve by cooling: besides, we observe the same phenomena with a very acid acetate, and also in vessels hermetically closed. Since then it is not owing to the volatilization of the acid, it is clear, that it must be occasioned by the heat: which, separating the particles of alumine and acid to a greater distance, carries them beyond the sphere of their action on each other, and occasions their separation: but, if the heat be diminished, these same particles enter again within their sphere of activity, and combine. This decomposition appears to me analogous to that of a neutral solution of carbonate of potash, or of soda, by heat; with this difference only, that the carbonic acid, being separated from its base, immediately flies off on account of its elasticity, and its little solubility in water; while the acetic acid remains still in presence of the alumine, because it is not volatilized by the temperature that occasions its separation.

Attempts to account for it.

Supposed analogous to the decomposition of the neutral alkaline carbonates,

and the coagulation of white of egg.

It appears to me also, that this decomposition has considerable analogy with the coagulation of albumen by heat: for, according to the explanation, which Mr. Thenard has given of this phenomenon, it is owing to the tendency water has to evaporate. Thus it happens in like manner, that the particles of water and albumen are carried by the heat out of their sphere of activity, and separate. No doubt they would combine again on cooling, in the same manner as the elements of the acetate of alumine; but water is too feeble a solvent, and the coherence the albumen has acquired too great, for the solution to take place.

VIII.

METEOROLOGICAL JOURNAL.

	Wind	PRESSURE. Max	Min	Med	TEMPERATURE. Max	Min.	Med	Evap	Rain	
11th Mo.										
Nov. 7	N E	29 58	29 43	29·505	52	41	46 5	—	·07	
8	S	29 65	29 35	29 500	52	42	47 0	·06	·19	☽
9	Var	29 67	29·31	29 505	54	47	50 5	—	·34	
10	S W	29 34	29 22	29 280	55	39	47 0	·10	·21	
11	N W	29 91	29 22	29·565	52	36	44·0	—	·03	
12	N W	29 60	29 50	29 550	54	38	46·0	·18	·22	
13	W	29 73	29 69	29 710	52	32	42 0	—		
14	S W	29 65	29 57	29·610	55	40	47 5	·12	·07	
15	W	29 57	29 49	29·530	48	36	42 0	·11	·06	
16	N W	29 66	29 49	29 575	50	41	45·5	—		●
17	S W	30 17	29·66	29 965	49	34	41·5	—		
18	N	30 25	30 17	30 210	50	45	47·5	—	·01	
19	N	30 29	30 25	30 270	53	31	42·0	·12		
20	N W	30 39	30·32	30·355	48	28	38 0	—		
21	S W	30 32	30 25	30 285	46	29	37 5	·06		
22	E	30 22	30 18	30 200	45	25	35 0	—		
23	N W	30·24	30 22	30 230	47	28	37 5	·06		☾
24	S W	30·38	30 21	30 295	46	33	39 5	—		
25	N W	30 36	30 35	30 355	50	38	44 0	·05		
26	W	30 41	30 35	30 380	47	41	44 0	—		
27	S W	30 40	30 35	30 375	44	40	42 0	·06		
28	W	30 35	30 27	30 310	45	42	43 5	—		
29	S W	30 27	30 25	30 260	47	42	44 5	—		
30	S W	30 25	30 10	30 175	50	41	45·5	·08		○
12th Mo.										
Dec. 1	S W	30·10	29 50	29 800	52	48	50·0	·15		
2	S W	29 86	29·50	29 680	52	31	41 5	·08	1	
3	S W	29 80	29 38	29 590	50	40	45·0	·12	·04	
4	W	29 35	29 21	29 280	46	31	38·5	—		
5	N	29 90	29 74	29 850	32	22	27 0	—		
6	S W	29 74	29 56	29 650	50	28	39 0	·15		
		30 41	29·21	29 898	55	22	42 95	1·49	1·41	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Eleventh Month. 7. A calm pleasant day. 8. Cloudy, drizzling. 9. Wind, a.m. N.W.: a dripping mist: then clear and calm: cirrostratus, evening: rain before nine the next morning. 10. *Nimbi* at sunset, with red haze on a brilliant twilight. 11. At sunset the clouds coloured in the E.: a *nimbus* in the W.: windy night. 13. A clear sunset beneath dense clouds. 14. Windy, S.W. a.m. 15. Clear, with *cirri* [illegible] at sunset to S. 18. Dripping mist. 19. Fair: *cumuli* p.m. which evaporating at sunset, a beautiful red twilight ensued, with *cirrostratus*. 20. a.m. Hoar frost and [illegible]

[illegible] atmosphere: twilight milky and luminous, with a blush of red. 23. Much rime on the grass, &c.: the sun emerged suddenly from the surface of a dense horizontal mist, [illegible] stretching from E. to W.: *cirrostratus* and *cumuli* beneath: the evening quite overcast. 24. Various modifications of cloud ending in *cumulostratus*. 25. Morning twilight red. 26. Calm: lightly clouded. 27. Overcast: a few drops p.m. 29. At sunset, a *stratus*, with a veil of superior clouds on the western sky richly coloured, the reflection from which gave considerable colour to the *stratus* itself: wind above, N.W. 30. Cloudy. The weather has been calm since the 15th inst.

Twelfth Month. 1. This morning the wind rose, bringing much cloud, with a few drops of rain: the night was stormy: and the evaporation was increased near sixfold: hence the formation of so great a mass of cloud, the superior atmosphere not being in a state to take up the water. 2. Rain commenced soon after eight a.m.; about this time too the thermometer, which had been rising, began to fall, the barometer, which had been descending, to rise; and the wind, which had been S.W., to go to the N. 3. Wind, a.m. fresh at S.W.: the sky overcast, chiefly with *cirrostratus*: stormy night: a shower about one a.m., after which the wind abated. 4. Clear, windy, a.m.: various clouds p.m. 5. Snow early this morning: wind N.: evening twilight orange coloured, but with fainter horizontal streaks of cloud above it, which were also discernible at the ensuing sunrise, with *cirrostratus* beneath: windy.

RESULTS.

Barometer: highest observation 30.41 inches; lowest 29.21 inches;
Mean of the period 29.808 inches.

Thermometer: highest observation 55°, lowest 22°;
Mean of the period 42.05°.

Evaporation 1.40 inches. Rain on the surface of the Earth, 2.41 inches: at [illegible] feet elevation 0.97 inches.

Wind chiefly S.W. and N.W. The fore part of the period wet, the middle fair and tending to frost, the conclusion windy and changeable. There has been a strong tendency to the red refraction during twilight.

L. HOWARD.

PLAISTOW, *Twelfth Mo.* 9, 1811.

IX.

Observations on some of the Strata in the Neighbourhood of London, and on the Fossil Remains contained in them: by JAMES PARKINSON, *Esq., Member of the Geological Society*[*].

Fossil organic remains important to geology

THE study of fossil organized remains has hitherto been directed too exclusively to the consideration of the specimens themselves; and hence has been considered rather as an appendix to botany and zoology, than as (what it really is) a very important branch of geological inquiry.

Compared with living beings.

From a comparison of fossil remains with those living or extant beings, to which they bear the closest analogy, great resemblances and striking differences are at the same time perceivable. In some instances the generic characters materially differ, but in most they very closely correspond; while the specific characters are very rarely found to agree, except where the fossil appears to have existed at, comparatively, a late period. Of man, who constitutes a genus by himself, not a single decided remain has been found in a fossil state.

Chemical analysis accounts for their preservation

Chemical analysis has been called in to the aid of the naturalist, in order to account for the perfect state of preservation observable in remains organized with the most exquisite delicacy, and which there is every reason for supposing to have been readily decomposable in their recent state. From this investigation we learn the manner, in which these memorials of the old world, so interesting and so frail, have been preserved. Some have been impregnated with calcareous matter, others with siliceous, and others with iron or copper pyrites.

Their study should be connected with that of the strata.

But these facts, however important and interesting, cannot, when considered by themselves, add much to our knowledge respecting the formation and structure of the Earth. To derive any information of consequence from them, on these subjects, it is necessary, that their exami-

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[*] Trans. of the Geological Society, vol. I, p. 324.

nation

ation should be connected with that of the several strata, in which they are found*.

General facts.

Already have these examinations, thus carried on, taught us the following highly instructive facts. That exactly similar fossils are found in distant parts of the same stratum, not only where it traverses this island, but where it appears again on the opposite coast: that, in strata of considerable comparative depth, fossils are found, which are not discovered in any of the superincumbent beds: that some fossils, which abound in the lower, are found in diminishing numbers through several of the superincumbent, and are entirely wanting in the uppermost strata: that some fossils, occurring in considerable numbers in one stratum, become very rare in the adjacent portion of the next superincumbent stratum, and afterward are lost: that fossils of one particular genus, which exist abundantly in the lower strata, and occur in several of the superincumbent ones, are not found in the three highest strata; while one species of that genus, but which has not been found in a fossil state, exists in our present seas: and lastly, that most of the remains, which are abundant in the superior strata, are not at all found in the lower. These general facts lead us to hope, that geology may derive considerable assistance, from an examination of fossils, made in connexion with that of the strata to which they belong.

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* This mode of conducting our inquiries was long since recommended by Mr W. Smith, who first noticed, that *certain fossils are peculiar to and are only found lodged in particular strata*; and who first ascertained the constancy in *the order of superposition*, and *the continuity of the strata of this island*. It will appear from the following quotation, that these observations have lately also occurred to Messrs Cuvier and Brongniart, while examining into the nature of the strata of the neighbourhood of Paris. "Cette constance dans l'ordre de superposition des couches les "plus minces, et sur une étendue de 1. myriamètres au moins, est, "selon nous, un des faits les plus remarquables que nous avons constatés "dans la suite de nos recherches. Il doit en résulter pour les arts et "pour la géologie des conséquences d'autant plus intéressantes, qu'elles "sont plus sûres.

"Le moyen que nous avons employé pour reconnoître au milieu d'un "si grand nombre de lits calcaires, un lit déjà observé dans un canton "très-éloigné, est pris de la nature des fossiles renfermés dans chaque "couche, ces fossiles sont toujours généralement les mêmes dans les "couches correspondantes, et présentent des différences d'espèces assez "notables d'un système des couches à un autre système. C'est un signe "de reconnoissance qui jusqu'à présent ne nous a pas trompé." Annales du Muséum d'histoire naturelle, tome XI, p. 07.

Strata in the vicinity of London.

The following is an attempt to investigate on this plan some of the upper strata in the vicinity of the metropolis with their contained fossils; and, although by no means complete, it will, it is hoped, induce others, who possess superior abilities and opportunities, not only to reexamine more correctly these strata, but to extend their researches to the subjacent strata.

The British strata have been considerably disturbed,

The whole of this island displays evident marks of its stratification having, since its completion, suffered considerable disturbance, from some prodigious and mysterious power. By this power all the known strata, to the greatest depths that have been explored, have been more or less broken and displaced; and in some parts have been so lifted, that some of the lowest of these have been raised to the surface; while portions of others, to a very considerable depth and extent, have been entirely carried away*. From these circumstances great difficulties and confusion frequently arise in examining the superior strata: the counties however immediately surrounding the metropolis, as well as that on which it stands, having suffered least disturbance, are those in which an investigation of these strata may be carried on with the smallest chance of mistake.

but least near the metropolis.

Real alluvial fossils rarely seen near London.

Real alluvial fossils, washed out of lifted or original superior strata by strong currents, and which in other parts are very abundant, are rarely seen in the counties adjacent to the metropolis. This remark is rendered necessary, since those widely extended beds of sand and gravel, with sandy clay, sometimes intermixed and sometimes interposed, and which have been generally hitherto considered as alluvial beds, are here assumed to be the last or newest strata of this island, slowly deposited by a preexistent ocean; with

Its beds of sand and gravel not alluvial.

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* See several essays on this subject in the Philosophical Magazine, by Mr. Farey, and the Report on Derbyshire, vol I, p. 105.

Also a Letter on the alterations, which have taken place in the structure of rocks, on the surface of the basaltic country in the counties of Derry and Antrim, by William Richardson, D D. Phil. Trans. 1808: or Journal, vol. XXII, p. 161, 245.

the strata, therefore, of this formation, these remarks commence.

Beds of Sand and Gravel.

Beds of sand and gravel.

The sands of this formation vary in colour from white, which is most rare, through different shades of yellow up to orange-red: the colour proceeding partly from a ferruginous stain on the suface of the particles of sand, and partly from the intermixture of yellow oxide of iron. Particles of those sands, which are disposed in distinct seams or beds, when examined by the microscope, are found to be transparent, most of them angular, but some a little rounded, with all their surfaces smooth, having no appearance of fracture, and resembling, in every respect, a uniform crystalline deposition. Those sands on the contrary, which, blended with broken and unbroken pebbles, form gravel, appear, when thus examined, to be mostly opaque, to be variously coloured, and to be marked with conchoidal depressions and eminences; the result of fracture.

Pebbles of this formation. 1st kind.

The pebbles of this formation appear to be of four kinds.

1st. Various pieces of jasper, gritstone, white semitransparent quartz, and other rocks. These have acquired, in general, smooth surfaces and roundish forms, evidently from attrition, and exhibit no traces of organization, except when, as is very rarely the case, the substance of the pebble is jasperised wood. The white quartz pebbles, like quartz crystals, on being rubbed together, emit a strong white lambent light, with a red fiery streak on the line of collision, and an odour which much resembles that of the electric aura.

Its organic remains rare, and only wood.

2d kind.

2d. Oval, or roundish, and rather flat siliceous pebbles, generally surrounded by a crust or coat differing in colour and degree of transparency from the internal substance, which also varies in different specimens, in these respects, as well as in the disposition of the parts of which the substance is composed. In some this is spotted, or clouded, in very beautiful forms; in others it is marked by concentric striæ, as if the result of the successive application of distinct laminæ: the prevailing colours in most of these pebbles being different shades of yellow. In several the traces of marine remains are observable: these are, in some, the casts, of

Marine remains found in several.

anomiæ, and the impressions of the spines and plates of *echini*; and in others, which generally possess a degree of transparency, the remains of *alcyonia*. The impressions, though frequently on the surface of the pebble, seldom, if ever, appear to be in the least rubbed down; thus seeming to prove decidedly, that these pebbles have not been rounded by rolling; but that they owe their figures to the circumstances under which they were originally formed. It is apprehended, therefore, that these pebbles have each been produced by a distinct chemical formation, which, it may be safely concluded from the remains of marine animals so frequently found in them, took place at the bottom of the sea, while these animals were yet living.

The pebbles not rounded by rolling

The formation of these fossils at the bottom of a former sea, and perhaps on the identical spots, in which they are now frequently found, is more plainly evinced by pebbles agreeing in some peculiar characters being found together in particular spots. Thus those in the county of Essex, ten miles northward of London, contain a much greater proportion of argil and iron, than those met with in many other places; hence their colours are darker, and the delineations, which their sections display, are very strong and decided, sometimes closely agreeing with those seen in the Egyptian pebble *. Passing on into Hertfordshire, pebbles of a very different character are found; their crust is nearly black, and their section displays delicate tints of blue, red, and yellow, disposed on a dead-white ground in very beautiful forms. In another part of the same county, occurs the pebble of the pudding-stone, which also presents peculiar characters of colour, &c.

Pebbles of similar character found together.

3d. Large tuberous, or rather ramose, irregularly formed flints, somewhat resembling in figure the flints which are found in chalk, materially differing however from them, not only in the colour of their external coat, which is of various shapes of brown; but also in that of their substance,

[illegible] flints [illegible] those in chalk.

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* The gravel pebbles of Epping Forest are of this description, and on most of the grounds leading down from the forest to the hamlet of Sewardstone, and to the town of Waltham, white, opaque, and partly decomposed pebbles are frequently seen, in which the argil and iron have been removed, and the silex only has remained.

stance, which is seldom black, but exhibits shades of yellow or brown, in which red likewise is sometimes perceptible. The traces of organic structure, particularly of the *alcyonium*, occasionally seen in these stones, determine them also to have been formed at the bottom of the sea.

Organic traces in them.

4th. Pebbles, owing their form to an investment and impregnation with silex, of various marine animals of unknown genera, but bearing a close affinity to the *alcyonia*. These stones display, in general, not only the external form, but the internal structure also of these animals. The congregation of many pebbles of this genus, and indeed of the same species, in particular tracts, warrants the conclusion, that these animal substances were thus changed, while inhabiting the bottom of a former ocean, which now forms the stratum, the contents of which are here sketched. Pebbles of this description are most frequently found in the gravel pits of Hackney, Islington, &c.

4th kind. Marine animals impregnated or invested with silex,

while at the bottom of the sea.

Among the traces of organization discoverable in this stratum are casts of *echini*, which are frequently found among the gravel, and which have generally been supposed to have been washed out of the chalk. But these casts have their origin plainly stamped on them. Their substance is covered with iron; they are almost always of a rude and distorted form; and I apprehend, that they are never found with any part of the crust of the animal converted into spar adherent to them, as is commonly the case with the casts of echini found in chalk.

Casts of echini,

not washed out of chalk.

A sufficient proof, that these several strata of gravel, sand, &c. have been deposited by a former ocean, is to be found in a circumstance, which does not appear to have been hitherto sufficiently adverted to. This circumstance is the existence of fossil shells belonging to, and accompanying, the superior part of these strata in particular spots: their absence in other parts being, perhaps, attributable to the removal of the upper beds.

Fossil shells accompanying these strata,

These fossil shells are still found disposed over a very considerable extent. Their nearest situation to the metropolis is at Walton Nase, a point of land about sixteen miles S. E. of Colchester. Here a cliff rises more than fifty feet above high water mark and the adjacent marshes. It is

disposed over a considerable extent now.

At Walton Nase.

formed of about two feet of vegetable mould, twenty or thirty feet of shells, mixed with sand and gravel, and from ten to fifteen feet of blue clay. The bed of shells is here exposed for about three hundred paces in length, and about a hundred feet in breadth.

Harwich cliff,

Immediately beyond the Nase the shore suddenly recedes and forms a kind of estuary, terminated towards the east by the projecting cliff of Harwich, which is capped in a similar manner with beds of these shells. The height of this cliff is from forty to fifty feet, about twenty-two feet of the lower part of which is the upper part of the blue clay stratum, "above which", as Mr. Dale observes, "to within "two feet of the surface, are divers strata of sand and "gravel mixed with fragments of shells, and small pebbles, and it is in some of the last mentioned strata, that "the fossil shells are imbedded. These fossils lie promiscuously together, bivalve and turbinate, neither do the "strata in which they lie observe any order, being sometimes higher and sometimes lower in the cliff; with strata "of sand, gravel, and fragments of shells between. Nor "do the shells always lie separate or distinct in the strata, "but are sometimes found in lumps or masses, something "friable, cemented together with sand and fragments, of a "ferruginous or rusty colour, of which all these strata are*."

and through Suffolk and great part of Norfolk

The coast of Essex is here separated from that of Suffolk by the river Stour, by which the continuity of this stratum is necessarily interrupted. It however occurs again on the opposite side of the river, and through Suffolk and great part of Norfolk the same bed of shells is found on digging; thus appearing to extend over a tract of at least forty miles in length.

Sometimes they are [illegible] some times th[illegible] [illegible]d.

These shells are in general found in the same confused mixture, as is described by Mr. Dale; but they are also sometimes so disposed, that patches of particular genera and species appear to be occupying the very spots where they had lived. This seems particularly the case with the small *pectens*, the *mactræ*, and the *left-turned whelk*.

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* Appendix by Samuel Dale to the History and Antiquities of Harwich and Dovercourt by Silas Taylor, 1732

From the excellent state of preservation, in which many of these shells have been found, it has been thought, that they could hardly be regarded as fossil. Many acknowledged fossil shells however have undergone much less changes than those of this stratum; the original coloured markings are entirely discharged, and the external surfaces are deeply penetrated with a strong ferruginous stain; the inner surfaces also are considerably changed, their resplendence being superseded, to a considerable depth, by a dead whiteness, the consequence of the decomposition of this part of the shell.

In excellent preservation.

Like the fossils of most other strata this assemblage of shells manifests a peculiar distinctive character. A few shells only, which may be placed among those which are supposed to be lost, or among those which are the inhabitants of distant seas, are here discoverable; the greater number appearing not to differ specifically, as far as their altered state will allow of determining, from the recent shells of the neighbouring sea.

Mostly do not differ specifically from those of the neighbouring sea.

Among those, of which no recent analogue is known, appears to be the *terebratula*, figured in Dale's History and Antiquities of Harwich, &c. tab. XI, fig. 9, p. 294, and described, Phil. Trans. No. 291, p. 1573. Mr. Dale describes this shell as *Concha longa fossilis fasciata*, and remarks, that he has not observed "either in Aldrovandus, "Rondeletius, Belonius, Gesner, Johnson, Lister, or "Bonanus, any shell, that resembles this our fossil, unless "it is one of those figured by Lachmund, p. 43, No. 6 "and 7, the inward part resembling our fossil." The shells figured by Lachmund are undoubtedly *terebratulæ*, but they manifest no particular agreement with this fossil.

A shell resembling no recent one known.

This shell appears to be figured by Lister, *Histor. Conchyl. tab.* 211, *fig.* 45, and is assumed by Gmelin, as *anomia spondylodes*. The other shells, fig. 46, of the same plate, referred to by Gmelin as *anomia psittacea*, appear to be mutilated specimens of the same shell. This opinion is corroborated, by the tint given by the accurate artists to the whole of the shells contained in this plate agreeing with the dark colour of the Essex fossil; and by the circumstance of their being generally found in the mutilated state, in which they are here figured by Lister. Besides, neither of Lister's specimens at all agrees with the pellucid shell, with a triangular foramen, of *anomia psittacea*; but they all agree with the oval antiquated shell, with an obtuse canaliculated beak, of *anomia spondylodes*.

Terebratula spondylodes.

In consequence of this agreement, it seems proper to consider this fossil shell as forming the species *anomia spondylodes*. But as the channelled beak is not natural to it, but is the consequence of injury; and as this part, in its natural state, is pierced with a large round foramen, a correspondent change should be made in the description, and it may be placed under the more appropriate genus of terebratula, as *terebratula spondylodes*, with an oval antiquated shell, the beak pierced by a large round foramen.

This shell is, in general, about an inch and a half long, thick, nearly oval, roughly striated transversely, and has its large foramen defined by a distinct border. It appears to differ from every known recent or fossil *terebratula*.

Another lost shell, ostrea deformis.

Another of the probably lost shells of this stratum is the fossil *oyster*, figured Organic Remains, &c., vol. III, pl. XIV, fig. 3; and which is there conjectured to be the same oyster, as that which is described by Lamarck as *ostrea deformis*.

Another, a volute.

The *volute*, Organic Remains, vol. III, pl. V, fig. 13, is another shell belonging to this stratum, of which it is believed that no recent analogue has been yet found. This ovate and rather fusiform shell appears to have been smooth; and at its full size about four inches in length; the columella has four folds, and the shell is formed by about six spiral turns, the last of which makes two thirds of the shell, dilating at about its centre, and contracting nearly equally upwards and downwards. The specimens yet seen give no opportunity of judging of the lip, or of the termination of the spire.

Reversed whelk.

The Essex *reversed whelk*, as it has been termed, *murex contrarius* Linn. *Hist. Conch.* of Lister, *tab.* 950, *fig.* 44, *b, c*, which is here very abundant, does not appear to be known in any other stratum of the island. The fossil shell, with the whirls in the ordinary direction, is sometimes found in this stratum*.

It has been said, that the recent analogues of both these shells are found in the adjoining sea. A recent shell is indeed found, which very nearly agrees with the ordinarily turned shell in its general characters: but there appears no authority for supposing, that the analogue of the left-turned variety has been discovered there.

Fossil shells of this stratum to be compared with recent ones.

Among those recent shells, the resemblance of which to the fossil ones of this stratum is such as appears to render a comparison by an experienced conchologist necessary, may be enumerated:

Patella ungarica, patella militaris, patella sinensis, (calyptræa, Lam.) *patella fissura, (emarginula,* Lam.) one or two species of *patellæ,* with a perforation in the apex, *(fissurella,* Lam.) *nerita glaucina, nerita canrena, (natica,* Lam.) *turbo terebra, (turritella,* Lam.) *murex corneus, murex erinaceus, strombus pes pelicani, cypræa pediculus,* with no sulcus along the back, *pholas crispatus,* in fragments, *solen ensis,* and *solen siliqua,* in fragments, *cardium edule, cardium aculeatum?* bearing the size and form of this shell, but having from thirty-four to thirty-six ribs, with no depressed line down their middle, nor vestiges of spines, *mactra solida, venus exoleta, venus scotica? venericardia senilis,* Lam., *arca glycemeris, arca nucleus.*

Other marine fossils.

Beside these remains of marine animals, the fossil hollow tubercles, having lost the spines, of the *thornback* are here found; also fragments of the *fossil palate, (scopula littoralis* of Lhwydd) and fossil remains of *sponge* and *alcyonia,* particularly a very fair specimen of the *reticulated alcyonium.* Org. Rem., vol. II, pl. IX, fig. 9.

Peculiar fragments of fossil bone.

In this bed, among the gravel and the shells, are frequently found fragments of *fossil bone,* which possess some striking peculiarities. They are seldom more than half an inch in thickness, two inches in width, and twelve in length; always having this flat form, and generally marked with small dents or depressions. Their colour, which is brown, light or dark, and sometimes inclining to a greenish tint, is evidently derived from an impregnation with iron. From this impregnation they have also received a great increase of weight and solidity; from having been rolled they have acquired a considerable polish; and on being struck by any hard body they give a shrill ringing sound. These fragments, washed out of the stratum in which they had been imbedded, are found on the beach at Walton, but occur in much greater quantity at Harwich.

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* It is erroneously stated, Organic Remains, vol. III, p. 66, that this shell has not been yet mentioned, as found in this stratum; since it is so particularised by Dale.

Part of a tooth of a mammoth.

Of the flat rounded pieces described above, no conjecture can be formed as to the particular bone, or particular animal, to which they belonged. But within these few years an Essex gentleman found, on the beach at Harwich, a tooth, which was supposed to have belonged to the *mammoth*. This fossil was kindly obtained at my request, for the purpose of being exhibited to the members of the Geological Society, by my late friend Dr. Menish; and certainly it appeared to be part of a tooth of that animal. It had been broken and rounded by rolling, but its characters were still capable of being ascertained. It possessed, in the softer parts, the colour and appearance of the Essex mineralised bones so distinctly, as to leave not a doubt of its having been imbedded in this stratum; while in the enamel it manifested decided characters of the tooth of some species of the mammoth, or *mastodon* of Cuvier.

Extent of this stratum.

The actual limit of this stratum has not been ascertained; it is however known to extend through Essex, Middlesex, part of Kent, and Surry, and through Hertfordshire, Buckinghamshire, and indeed much farther both to the northward and westward. In many parts its continuity has been interrupted, apparently by partial abruptions of it, together even with a portion of the stratum on which it rests. The shells of this stratum have hitherto been discovered only in the parts already noticed.

Blue clay stratum.

Blue clay stratum.

This, the next subjacent bed, is formed of a ferruginous clay exceeding two hundred feet in thickness. Its colour for a few feet in the upper part is a yellowish brown, but through the whole of its remaining depth is of a dark bluish gray, verging on black. It is not only characterised by these circumstances, but by the numerous septaria, which are dispersed through it, and by the peculiar fossils, which it contains.

Cause of the difference in its colour.

The difference of colour observed between its superior and inferior part, and which has generally been supposed to be owing to a difference in the degree of oxidation of the iron present in it, appears to be the result of a difference in the quantity of it, occasioned by the washing away of this metal in the upper part by the water which percolates through it, and which runs off laterally by the numerous drains made near the surface. The dark red colour of tiles made from the blue clay, the reddish-yellow colour of the *place* bricks made of the yellowish-brown clay, and the bright yellow hue of the *washed malms*, those bricks which are formed of the yellow clay which has been exposed to repeated washings, are thus accounted for.

Septaria.

The septaria lie horizontally, and are disposed at unequal distances from each other in seemingly regular layers; and, as has been just observed of the stratum itself, they become of a paler colour, and it may be added suffer decomposition, when placed so high in the stratum, as to be exposed to the action of percolating water. They frequently include portions of wood pierced by the *teredines*, *nautili*, and other shells; and it is a fact, that may be worthy of being attended to, while inquiring into their formation, that the septa of calcareous spar frequently intersect the substances enclosed in the septaria.

Their septa frequently intersect the substances enclosed in them.

Extent of this stratum.

This stratum is to be found not only wherever the preceding deposition extends, but in other parts also, where that has been removed. The cliffs of this clay, at Shepey, extend about six miles in length; the more elevated parts, which are about ninety feet in height, being about four miles in length, and declining gradually as they terminate towards the east and west.

Its fossils, in Shepey.

The fossils of this stratum have been already carefully particularised. A catalogue of those found at Shepey was added by Mr. Jacobs to his *Plantæ Favershamienses*: and an account of several of the fossil fruits found at Shepey was published by Dr. Parsons in the fiftieth volume of the Phi-

and in Hampshire.

losophical Transactions. The fossils of Hampshire have been scientifically described by Dr. Solander, in the *Fossilia Hantonensia* of Mr. Brander, where the fossils themselves are very exactly figured.

It was not supposed, even after the publication of these accounts, that the fossils of Shepey and those of Hampshire were of the same stratum. Among the Hampshire fossils no mention is made of *crabs*, *lobsters*, *tortoises*, *nautili*, or of the heads or bodies of *fishes* so abundant at Shepey, while the *murex pyrus*, *murex longævus*, *strombus amplus*, &c. of the Hampshire cliff had never, perhaps, been enumerated among the Shepey fossils.

The stratum in both places identical.

The identity of the stratum at Shepey and in Hampshire has, within a few years, been decided by digging into this same stratum at Kew, where several of the fossils, which had hitherto been supposed peculiar to Shepey, were found in the same pit with those which had been considered as peculiar to Hampshire.

Farther proof of it at Highgate.

In the present year, on cutting through a mound of this stratum which forms Highgate Hill, this identity has been still farther manifested by the discovery of great numbers of those fossils mingled together, which had been generally distinguished into Hampshire and Shepey fossils; as *crabs*, *nautili*, &c., like those of Shepey, together with several shells, which had been generally regarded as peculiar to Hampshire, and in particular that uncommon alated shell, *strombus amplus*, Solander. (*Rostellaria macroptera*, Lamarck.)

Certain organic remains peculiar to particular depositions.

In examining this stratum, the curious fact, that certain organic remains are peculiar to particular depositions, is first observed. Very few indeed of the fossil shells of the gravel strata are to be found in the bed of blue clay. In the gravel strata, by far the greater number of the shells bear a close agreement with those, which now exist in not very distant seas; but in this clay stratum, "very few of the shells are known "to be natives of our own, or indeed any of the European "shores; but the far greater part of them, upon a compa-"rison with the recent, are wholly unknown to us*."

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* Fossilia Hantoniensia, p. [illegible]

This stratum, though older than the former, yet modern

But although this clay stratum contains fossils of a much older date than those of the gravel stratum, it possesses other marks, which agree with its position in showing, that it is of comparatively modern formation. It includes none of the remains of any of the lost fossils, such as the *cornu ammonis*, *encrinites*, &c. Mr. Jacobs indeed speaks of one imperfect specimen of *belemnites* and of *astroitæ* having been found, but at the same time as being very uncommon; Mr. Lander however does not appear to have met with any of these older fossils; nor have any of them been discovered either at Kew or at Highgate. Hence it seems reasonable to conclude, that the single imperfect belemnite and the few astroitæ were not inhabitants of the sea at the period when this stratum was deposited, but were washed out of some of the more ancient strata, and lodged by accident in the bed where they were found*.

No remains of lost fossils in it

700 specimens of seed vessels

The quantity of fruit or ligneous seed vessels and berries, which has been found in this stratum at Shepey, is prodigious. Mr. Francis Crow, of Feversham, has procured from this fertile spot a very large collection: and, by carefully comparing each individual specimen by their internal as well as their external appearance, he has been enabled to select seven hundred specimens, none of which are duplicates, and very few agree with any known seed vessels. These vegetable remains have also been found on the opposite Essex shore, but in very small numbers. They have also been met with in that part of the stratum, which has been examined at Kew. At Highgate and at Shepey a resinous matter, highly inflammable, of a darkish brown colour, and yielding, on friction, a peculiar odour, has also been found. This substance has been conjectured, to exist in an unaltered state; and this indeed seems to be the fact from its resinous fracture: but it must be observed, on the other

Peculiar resinous matter.

Two sources of errour.

* It appears to be necessary to guard against two sources of errour, while appropriating fossils to their respective strata: one is the circumstance here alluded to, where the fossils of a preexistent stratum have been washed out by the waters while depositing a more recent stratum; the other is where, at the line of junction of two strata, the animals of the one are found within the borders of the other stratum; a circumstance by no means difficult to be conceived or explained.

hand, that pieces of it occur, which are penetrated by iron pyrites.

Land animals appear to have resided on it.

This stratum is also rendered exceedingly interesting by its surface appearing to have been the residence of land animals, not a single vestige of which seems to have been found in any of the numerous subjacent strata of the British series. Mr. Jacobs relates, that the remains of an *elephant* were found at Shepey. The remains of the *elephant, stag,* and *hippopotamus* have also been dug up at Kew. At Walton, in Essex, not only the remains of the *elephant, stag,* and *hippopotamus* have been discovered, but also remains of the *rhinoceros,* and of the *Irish fossil elk.* Org. Rem., vol. iii, p. 366.

Situation of their remains.

It has been generally supposed, that these remains were contained within the stratum of blue clay; but the circumstances, under which they are found, seem rather to warrant the conclusion, that they were deposited on the surface of those low spots, where abruptions of the superior part of this stratum had taken place. Thus the remains of the elephant mentioned by Mr. Jacobs were not in the cliff, but in a low situation at a distance from it; so also the remains of land animals in Essex occur a little below the surface, in a line with the marshes, which are a very few feet above high water mark. By a communication of the late Mr. William Trimmer, of Kew, it appeared, that he found, under the sandy gravel, a bed of earth, highly calcareous, from one foot to nine feet in thickness; beneath this a bed of gravel a few feet thick, containing water, and then the main stratum of blue clay. At the bottom of the sandy gravel, he observed, that the bones of the *hippopotamus, deer,* and *elephant* were met with; but not in those parts of the field, to which the calcareous bed did not extend. Here also a considerable number of small and apparently fresh-water shells, and, at the bottom, snail-shells were found. Does it not seem, that the first appearance, or creation, of land-animals was on the dry land of this stratum; and that they were overwhelmed in these spots, by that sea, which deposited the present superincumbent strata of gravel?

Sh[illegible] d here.

Commencement and destruction of [illegible]

(To be concluded in our next.)

X.

Various Observations respecting the Art of Glassmaking, with a View to explain some Phenomena, that occur in the Fabrication of Glass, and point out the Application of these to the obtaining of new Products: by Mr. GUYTON-MORVEAU*.

Art of glass-making empirical, but ought not to continue so.

THE art of glassmaking, though one of the most ancient, since there are documents that attest its having been practised by the Phenicians, was long, like most of the useful arts, nothing more than a tradition of those processes, which had most uniformly succeeded. Now, however, we feel the necessity of combining with it those principles, the application of which has successively unfolded the essential circumstances of the processes, increased and improved their products, and may yet afford new views either of economy or perfection.

Writers on it.

Such was the object Mr. Loysel proposed to himself in 1791, in the Essay presented by him to the Academy of Sciences; which, under that modest title, left far behind it the works of Nery, Merret, Kunckel, Haudiquert, Blancourt, and others, who had written on the subject.

D'Artigues.

Still more recently the labours of Mr. d'Artigues† have given us hopes of a treatise, that would embrace the whole of this art, and place it on a footing with the present state of our knowledge.

Facts noticed by the author.

The two papers, that Mr. d'Artigues has already communicated to the class, have called my attention to some facts, that I had noted down; and which appear to me sufficiently connected with the most important phenomena in the processes of this art, not to be consigned to oblivion. Of these I shall proceed to give a succinct account, with such reflections as may tend to elucidate their theory.

The chief subjects of these observations will be:

Classification of these.

1. The separation of glass of different densities by eliquation:

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* Ann. de Chim. vol. LXXIII, p. 113 Read to the Institute, the 29th of Jan. and 5th of Feb 1810.

† See Journal, vol. X, p p. 58, 89.

2. The results of the annealing of large masses in crucible-moulds.

3. The colouring of glass red by copper, and in cements.

4. The alteration glass undergoes by long-continued heat.

5. This alteration by the fire of our furnaces compared with that of volcanoes.

6. Lastly, what constitutes the real difference between transparent and devitrified glass.

Obs. I. *Separation of glass of different densities by eliquation.*

Experiments of Buffon.

In 1776 I accompanied Mr. de Buffon to the plate glass manufactory then existing at Rouelle, near Langres, under the direction of Mr. Allut, who wrote the article *glacerie* in the *Encyclopédie*. His object was to make some experiments on the fabrication of a mass of flint-glass, for constructing the *lentille à échelons* described in the first volume of his Supplements. I shall not speak of the various processes tried, and the difficulties that obliged him to give up the hope of obtaining one single piece of sufficient thickness, but confine myself to the very extraordinary result of one trial I witnessed, and which I conceive may be compared to what metallurgists term *eliquation*.

Glass separated into two strata as if by eliquation.

A mass of flint-glass*, 37 lin. [3·40 inches] thick, had just been run out on the copper table. A portion of this glass, about three or four fingers thick, was left in the crucible, and it was supposed, that, by charging it afresh with the common composition, the glass obtained would be so much the finer, because it would approach nearer in quality to flint-glass. The refined glass having been ladled into the cistern, and run on the table to the thickness of three lines, was placed in the annealing furnace. When it was taken out, its quality was examined, and, to our great surprise, on cutting it there appeared, instead of a single glass, two very distinct strata, the line of separation of which was plainly marked, and extended throughout the mass; the lower stratum occupying about one third of the thickness. I brought away a piece of it, which I showed at the public lectures of the Academy of Dijon, in the collection belonging to which it was deposited.

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* The composition was 39 parts powdered Madagascar crystal, 30 minium, 16 soda, and 1 nitre. Elem. de Chim. de Dijon, vol. I, p. 179.

The lead renders flint glass seldom uniform.

It was already well known, that glass, in the composition of which a large proportion of oxide of lead is employed, does not easily afford a homogeneous mass, because the denser parts are not retained by an affinity capable of producing an equilibrium; and hence the difficulty of obtaining flint-glass free from streaks. But such a speedy and complete precipitation is a solitary instance, arising from a combination of circumstances, which we can scarcely hope to reproduce.

Its streaks a commencement of eliquation.

From what has been said, we can scarcely doubt, that the streaks, from which glass abounding in oxide of lead is seldom free, arise from a commencement of eliquation. The horizontal position of these streaks proves it; for they are not distinctly perceptible, except the light comes to the eye in a direction parallel to the zones of unequal density. I have a piece of flint-glass, manufactured also under my own eye, which is three cent. [1·18 in.] thick, which any one, not apprised of the contrary, would suppose to be perfectly good, because the division is softened down.

Obs. II. *Trials of a crucible-mould for annealing large masses of glass.*

Stone mould for annealing large masses of glass

In the various experiments made at the plate-glass manufactory at Rouelle for the same purpose, a hard calcareous stone, cut in the shape of a circular crucible, was at first employed. It was supposed, that, when it had been converted into lime by a graduated fire, without having its shape altered, it would hold the refined glass; so that, bounding the bottom accurately, the glass would be annealed by cooling slowly in it, as in an annealing furnace. The result was a mass full of large blebs throughout its substance, and on its surface.

Effects.

Pottery-mould.

With the same view trial was made of a crucible-mould made of the best potter's clay, and baked as hard as possible. The glass was perfectly refined in this, and retained its homogeneousness in annealing; but the mass, 7 cent.

Result

[2·75

[2·75 in.] thick, and 120 [47·2 in.] in diameter, was divided by cracks from the centre to the circumference; because the adhesion of the glass to the sides of the crucible had prevented it from contracting its dimensions, the clay, so highly baked, not being susceptible of an equal diminution of bulk in cooling. I preserve a piece of this, cut in the form of a *serre-papier*, the transparency of which in so great a thickness is remarkable, though the composition was not prepared for being colourless.

OBS. III. *Glass coloured red by copper.*

Red glass.

Hitherto glass has been stained red, whether for church windows, or for imitating gems, only by combining in different proportions, according to the tint desired, oxide of gold by tin, oxide of manganese, and sulphuret of antimony. Such are the compositions indicated by Fontanieu, and in Loysel's Essay on Glassmaking.

Glass stained red by iron,

Clouet has given a different process in his Inquiries into the Composition of Enamels, which he was so kind as to communicate to me in manuscript some years ago, and which I published in the Annals of Chemistry, for May, 1800. This process consists in fixing the colour of red oxide of iron, by calcining a mixture of sulphate of iron and sulphate of alumine: but he precisely declares, that we have no metallic oxide, which gives a red directly by fusion ······ that this colour must be composed of different substances······and that it is desirable to multiply experiments with the new metals, which would perhaps furnish a red, *that is not to be produced directly or easily by any of the metallic substances anciently known.* He speaks of the oxide of copper only in the preparation of green enamel: and though he sometimes obtained a tolerably fine red from it, particularly by mixing with it oxide of iron, he says, that this colour is very fugacious, and frequently disappears even while the glass is making.

and by copper

Red glass from copper by accident

An accident, that happened in 1783 at the plate glass manufactory of St. Gobin, appeared to me to ascertain the circumstances, in which we may hope to fix in glass the colour of red oxide of copper; and a direct experiment, made at the labgratory of the Polytechnic school, tends to confirm this conjecture.

Part of a ladle being melted in plate glass.

It is the practice in plate glass manufactories, when the glass is refined, to ladle it out of the pot into a cistern, which is afterward taken out of the furnace, that the glass may be run on a table. The ladles are made of copper, with an iron handle, and are dipped into water, as soon as they begin to get hot. A workman, having neglected this precaution, brought out only a part of his ladle. It was supposed, that the melted portion would sink to the bottom of the pot, and be preserved there as under a vitreous flux. Accordingly the casting and annealing were proceeded with as usual; but to the surprise of the workmen, the glass exhibited, not only a few metallic grains embedded in it, but bands pretty uniformly coloured of a very bright red. I lay before the Class a piece of this glass, polished on one side, 17 cent. [6·7 in.] long, 12 cent. [4¾ in.] wide, and 7 mil. [2·75 lines] thick.

Theory.

There can be no doubt, that this colour was produced by the copper carried suddenly to that degree of oxidation, which gives it this property; and fixed in this state by its diffusion through the vitreous mass. But can we be certain of reproducing the same circumstances? and by what means? This I was desirous of ascertaining by experiment.

Can we produce the effect at pleasure?

Experiment with plate glass,

I took some powdered plate glass, mixed it with three per cent of copper filings, and brought the mixture to complete fusion. The glass was without colour, and the copper in metallic globules.

with common glass,

I repeated the experiment with common white glass and six per cent of copper filings; and obtained a vitreous mass, well fused, and of a very uniform red colour, but so deep, that it appeared in the state of enamel rather than of glass. On the surface was observable a crust less compact, approaching to the nature of scoriæ, of a brown inclining to black.

and with oxyde of copper

Mixtures of glass and copper in the state of oxide, even in the lowest degree, afforded only a greenish tinge, and part of the copper was reduced.

These results, while they announce the possibility of producing a red glass with copper, confirm the opinion of Clouet respecting the difficulty of rendering this colour fixed in the fire. But why did plate glass afford only reduced copper, while common white glass produced a vitreous oxide? It seems to me, that it would be difficult to account for it by supposing, that the latter contained some oxigenating substance: but it offers itself naturally when we consider, that the composition of the former, being much more fusible, occasioned the fusion of the metal, and thus withdrew it from the action of the air, before the temperature was sufficiently high to be effectual.

Attempt to account for the difference.

It is unnecessary to observe, that this explanation is not inconsistent with the phenomenon before described, since the spoon did not pass to the state of vitreous oxide in the plate glass, till it had repeatedly undergone the action of the air and of the heat of the furnace simultaneously.

Mr. d'Arcet has made several trials for colouring glass with cements impregnated with colouring metallic oxides. He employed iron, copper, cobalt, and manganese, in various proportions, and in different states. Iron left but pale colour. Cobalt and manganese coloured only the cements. In that made with copper left from the distillation of its acetate, the glass was completely devitrified, and of a deep green at its surface, the colour growing lighter toward the centre, where it had a reddish tinge. A plate of glass coloured by cobalt having been placed in the common cement with a capsule of white glass, and exposed to a heat of 50° of Wedgwood; part of the capsule was found to be tinged blue, without having undergone fusion, the surfaces being only divested of their polish, and a little roughened; which is readily accounted for by the known property of this metallic oxide to rise in vapour at a very high temperature.

Attempts to colour glass with metallic cements.

OBS. IV. *Of the alteration that glass undergoes by the action of great heat long continued.*

The interesting paper of Mr. d'Artigues on the devitrification of glass* has turned men's opinions toward the

Devitrification of glass

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* Ann. de Chim. vol. L, p. 225: or Journal, vol. X, pp. 58, 89.

real cause of this phenomenon, which was too long considered as the product of a cementation according to Reaumur's process*. Certain facts, which I noticed long ago, may furnish some particulars illustrative of the explanation he has given.

by long continued heat.

Specimens, by Mr. Ciffle.

In 1782, Mr. Ciffle, a porcelain manufacture at Lunéville, sent me several specimens of glass of different qualities, rendered opake by the long-continued action of heat, without having been surrounded by a mixture of sand and gypsum in Reaumur's mode. The five pieces, which I lay before the class, made part of these specimens, and have the original labels still fastened to them.

No. 1.

No. 1 is a piece of common window glass, 13 cent. by 10 [5 in. by 3·9†], which, by exposure to the strong heat of a porcelain furnace, without any cement, has become absolutely opake, and very white, without any alteration of its shape; and has acquired much greater hardness and solidity.

No. 2.

No. 2, a piece of the same window glass, exposed in the same furnace, and touched by the flame, has also become opake, and of a fine white in the fracture; but the surface has a yellowish tinge.

No. 3.

No 3 is a piece of bottle glass, kept in the fire in charcoal dust, equally become opake, of a fine white internally, with a uniform shining coat of brown black over all its surface.

No. 4.

No. 4 is a piece of a bottle, which has undergone the heat of a porcelain furnace surrounded with powdered soot. It has acquired a coat of a deep lustre-colour, but within is completely devitrified, and equally white.

No. 5.

No. 5 is the bottom of a bottle, which was exposed to the most violent heat without being surrounded with any thing, and has become white and opake throughout its whole thickness.

Glass devitrified without cement by Mr. Keir.

At the time of these experiments by Mr. Cifflé, and indeed some years before, Mr. James Keir had announced, that glass might be rendered opake by long continued annealing, without any cement, and that in this state it was more dense, and less liable to break by a sudden transition from cold to hot, or the contrary. The latter property was confirmed by the experiments of Mr. Ciffle, so that he did not hesitate to consider glass so altered as the most proper substance for supplying chemistry with vessels at once refractory and not liable to crack.

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* Mem. de l'Acad des Sciences, 1739.

† It was longer when it came to my hands, but I reduced it to these dimensions, in order to make experiments with some pieces of it.

The effect ascribed to crystallization.

Mr. Keir, after having described these phenomena, ascribed it to the crystallization of the vitreous matter, an opinion naturally arising from the aspect of the fracture, which, instead of being conchoidal, as in transparent glass, exhibits, if not facets, at least very decided parallel striæ.

This supported by the experiments of d'Artigues.

The observations of Mr. d'Artigues strongly support this explanation. I have myself a mass of glass, found five years ago at the bottom of a crucible at the manufactory of St. Gobin, which appears formed to afford a demonstration of it; since we can distinguish, even with the naked eye, prisms shooting from the devitrified crust that constitutes its surface, and which is 2 or 3 mill. [about a line] thick.

[illegible] and is there a precipitation?

Is it true however, that all these changes are solely the effect of a crystallization? and can we admit with Mr. d'Artigues, "that a precipitation takes place throughout the mass, each of the component parts obeying at the same time the laws of attraction"? Before I attempt to solve these questions, I shall add a few more facts, resulting from experiments on this subject made by Mr. d'Arcet, and the consequences of which will naturally find a place in this discussion.

Experiments by d'Arcet.

Specimen 1. Bottle glass completely [illegible]

Among the ten specimens from these experiments, which he put into my hands, No. 1 is a piece of bottle glass, which was exposed for three days to a heat of 50 of Wedgwood in Reaumur's cement. The devitrification is complete; interiorly it has a rosy tinge; the fracture exhibits striæ, arranged in stars, to the very centre; it gives no signs of electricity by friction; it rather scratches rock crystal, than is scratched by it; corundum leaves a mark on it scarcely perceptible through a lens.

[illegible] 2. [illegible]

No. 2, exposed to the same fire, the same time, in the same cement, has barely acquired an earthy crust, which is scratched by rock crystal. The interior has remained of the nature of a green glass, transparent, and forming a geode by the contraction of the matter, while adhering to the crust. This glass contained oxide of lead.

A geode.

Specimen 3. Artificial intaglios of bottle glass.

No. 3 includes two artificial intaglios. They are made of bottle glass, moulded first in a cupelling furnace on an impression taken with tripoli, and then devitrified at a heat of 51° of Wedgwood. They are not electric by friction even on the polished faces. Their specific gravity is 2·801. Corundum scarcely leaves a visible trace on them. This hardness, which is such that it allows them to be used as dies, and the chastity with which impressions from them represent antiques, have introduced the products of these trials among the ornamental arts; and this not only for intaglios, but also for cameos, the figures of which are now executed of a different colour from the ground, by the addition of coats of glass of a different composition, to imitate onyxes; devitrification being afterward employed, to give them that hardness, which is the principal characteristic of precious stones. I shall say no more of this new art, the processes of which, we may easily foresee, will be improved by that industry, which will also extend its applications.

Cameos.

Specimen 4. A capsule.

No. 4 is a piece of a globe of the same glass, cut to be used as a capsule, and afterward devitrified in Reaumur's cement at 50° Wedg. The fragments with it, being from a similar capsule, show the striated fracture. Corundum scarcely leaves a visible mark on them, and they are not sensibly electric by friction. These fragments may be heated red hot in the fire, and immediately thrown into water, without losing any thing of their solidity. I have kept them in sulphuric acid in the strongest fire, and they have come out without the least alteration, or loss of weight.

Specimen 5. Effect of slow refrigeration merely.

No. 5 is remarkable for the differences it exhibits. It has still the vitreous fracture; is evidently translucid at the edges; becomes electrical by friction; and is scratched by silex. Accordingly it differs from bottle glass only by the appearance of a grayish white porcelain or enamel, which it has acquired in losing its transparency. But these differences may be accounted for by the process employed in this experiment, the object of which was, to learn what would be the result of slow refrigeration alone. It is evident, that the heat was not carried to a sufficient height, or that it was not continued long enough, to complete the devitrification.

Specimens 6, 7 and 9 Stained glass.

Red.

No. 6, 7, and 9, are the results of trials to devitrify pieces of stained glass from church-windows, coloured red by oxide of gold, and blue by oxide of cobalt. The first two, in losing their transparency, have acquired a purple tinge but one of them, which had lead in its composition, had but little consistency, and was interiorly full of blebs, and as it were spongy; while in the other the devitrification had pursued its usual and regular course from the two surfaces, leaving in the middle a remainder of vitreous matter, which would have disappeared on a longer continuance of the fire. This exhibits some signs of electricity by friction, and both are scratched by rock crystal.

Blue.

The fragment stained by cobalt announced by the aspect of its fracture, which was still a little vitreous, that the devitrification was not far advanced. It had lost all its transparency however and its blue colour, though weakened in the mass, was much more intense at the surface. It was still a weak insulator. Its hardness was such, that corundum scarcely made a perceptible impression on it.

Specimen 8. Farther proof, that devitrification begins at the surface

No. 8 is remarkable as a fresh proof, that devitrification always commences at the surfaces, and proceeds gradually to the centre, when the heat is continued long enough. This piece resembles a small geode, the crust of which, completely devitrified, includes a portion of the substance still in the state of perfect glass. We shall find, that these accidents equally occur in the devitrifications by the fire of volcanoes.

Specimen 10. Attempt to form an intaglio, after the glass was devitrified

No. 10 exhibits a result still more interesting. It is an attempt to form an intaglio, not by moulding it in the state of glass, and devitrifying it afterward; but by devitrifying it previous to its being placed on the mould, to receive the impression. The fusion has produced a very homogeneous mass, of a dull gray colour, which exhibited, though imperfectly, an impression of the figure in relief on which it had been cast, while its completely vitreous fracture.

ture, and translucency on the edges, evidently announced a return to the state of glass, as far as it could in the proportions of its present composition.

From these characters I could not avoid suspecting, that there must be a correspondent change in the specific gravity. This was fully proved on trial, for that of the mass thus brought back to the vitreous state was only 2·625, while that of the same glass, when completely devitrified, was always from 2·77 to 2·801.

Mr. d'Artigues has rightly observed, that glass, when devitrified, is not so bad a conductor of heat and electricity as before. In fact we have seen, that several pieces of different kinds of glass, when brought to this state, no longer exhibited any sign of electricity by friction. If it were possible to doubt, that this property depended more on the nature of the component parts, than on the manner of their arrangement, we should be obliged to return to this principle from the result of the experiment, in which devitrified glass, restored to its former state by being fused again without addition, and having resumed its original density, fracture, and other characters (except the transparency, which appeared only on the edges), showed no more disposition to become electric by friction than before.

Electricity depends more on the nature of the principles, than on their arrangement.

All the products of devitrification I have hitherto mentioned concur in showing, that it commences constantly at the surface: and this is a fact of sufficient importance for us to inquire into the real cause of such accidents, as may give rise to objections against this principle.

Devitrification always commences at the surface.

Are there in reality instances of a devitrification effected internally; or between two portions of unaltered glass? A plate from the glass-house of Premontré, given me by Mr. d'Arcet, appears, at first sight, to show the possibility of this. The part completely devitrified forms a very white stratum, absolutely opake, 5 or 6 mil. [2 or 2·4 lines] thick, between two strata, rather thicker, of green glass, which have retained all their transparency, and exhibit the vitreous fracture very decidedly contrasted with the striated fracture of the devitrified part.

A specimen seemingly contradicting this.

But, on a careful examination of this piece, we soon perceive, that it was not cooled at rest; and that a portion of

But in fact a proof of it.

the glass, which was still fluid beneath the superficial stratum, that had become opake and more refractory, was carried above it by the motion of the crucible, in taking it out of the furnace. A comparison of it with another plate of the same glass, in which we find only the two strata in their natural order, appears to me to leave no doubt of the truth of this explanation.

OBS. V. *Devitrification of glass by the fire of volcanoes.*

Supposition that volcanic differ from common fires.

The hypothesis framed by the celebrated Dolomieu is well known: that the fires of volcanoes do not act like those of our furnaces; that, though they produce prodigious effects, their activity is not great; that the fluidity they occasion is not that of vitrifying matter; and lastly, that even the most fusible substances, included in the body of rocks, might have flowed in burning torrents, without having undergone any perceptible alteration*.

Supposed proof of this in glass buried in lava at Torre del Greco.

He imagined he had found a proof of this in the state to which pieces of glass had been reduced at the time of the dreadful eruption, that covered Torre del Greco, in 1794. This glass, the shape of which was still distinguishable, had become of an opake white. The alteration extended sometimes throughout its whole thickness: sometimes it left glass still untouched, with its original colour and transparency, between two opake crusts. Dolomieu laid before the class several specimens of these glasses, found in digging on the spot. He was so good as to present me with a few specimens, some of which were authenticated by volcanic scoriæ still adhering to them†: and I promised him in return several fragments, found in a furnace [four à étendre], where, as is too frequently the case for the manufacturer's

Similar effects in our glass-houses.

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* Journ. de Phys. vol. XXXVII, p. 195. Journ. des Mines, No. 22, p. 55.

† Mr. Breislak mentions in his Tour through Campania, vol. I, p. 240, a piece of window glass bent in different directions, the surfaces of which were converted into Reaumur's porcelain, while the interior retained the state and appearance of glass. Dr. Thomson, in his catalogue of substances found in the lava of 1794, had already described fragments of glass thus modified, to which he gave the name of glass-stone.

facturer's profit, the broken glasses are raised on the sides, to remain till the working ceases, or till their accumulation obliges it to be stopped for emptying the furnace; in which he would find the same alterations, and the same progress of devitrification.

which convinced Dolomieu.

Dolomieu, having seen them in my collection, with prof. Pfaff, of Kiel, who was then in Paris, frankly confessed, that he had nothing to object against the identity of the effects of the glass-house fire and the fire that had acted on the glasses found at Torre del Greco; and he selected a few specimens for his collection.

Experiments in confirmation by d'Arcet.

The fact, which authorizes us to compare the effects of the fire of volcanoes and of our furnaces at equal intensities, is supported by experiments, communicated to me by Mr. d'Arcet, which are equally interesting for their practical application, and the consequences deducible from them respecting the formation of basaltes.

Sir James Hall's experiments.

It is well known, that basaltes fuses about 60° Wedg.: and, as sir James Hall has very justly remarked, the product of this fusion is a glass, having all the characters and properties of volcanic glass*. I have obtained some myself, in pretty considerable masses, from the basaltic prisms of the extinct volcano of Drevin, which, after the operation, could not be distinguished from the glass produced by the fusion of hornblende rock, touchstone, or vitreous obsidian lava.

Devitrification of volcanic glass.

Mr. d'Arcet tried the processes of devitrification on volcanic glass itself. He subjected to it pieces of 15 or 16 cub. cent. [9 or 10 cub. in.], and of spec. grav. from 2·775 to 2·784; and observed, that they were completely devitrified in the fire of a cupelling furnace; but, if the heat were carried to 70° Wedg. only, part, that had been before devitrified, returned to the state of glass.

The results agreed with Sir J. Hall's.

I need not remark the conformity of these results with those, which sir James Hall obtained by the slow cooling of basaltes, which he had first converted into glass; and on which principally he founded his opinion, that basaltes had been originally in a state of vitreous fusion.

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* Journ. de Phys. germinal, an 7, p. 317. [See Journal, 4to ed. vol. IV, pp. 8, 56.]

Artificial touchstones of the best quality.

The volcanic glass, thus brought back to the state of very compact and very fine-grained lithoid lava, induced Mr. d'Arcet to have some polished, to serve as touchstones; and the trials he made leave no doubt, that they would supply the place of the native stones of the best quality, which are becoming very scarce.

Examination of what constitutes the real difference between transparent and devitrified glass.

[illegible]

Can the facts I have recorded be explained by simple crystallization? or, to express myself more clearly, can they be reconciled with the known effects of this transition of substances to the regular concrete state, and with the hypothesis of a simultaneous precipitation of some of their mixed elements? This remains for me to examine.

[illegible]

In the first place, we may observe, that, if there were a crystallization and precipitation at the same time, the opake mass resulting from these would no longer be crystallized glass, but the product of its decomposition.

[illegible]

In the next place, if there were in reality a separation of some of the ingredients of the glass, they should exhibit, in some parts at least, the appearance of the colour, the degree of hardness, and the other characters peculiar to them, of which we do not perceive the least indication.

[illegible]

Lastly, on this supposition, the state of combination having ceased, the particles abandoned should be immediately given up to the chemical action of their solvents: but it is certain, that nothing is taken up from devitrified glass even by the most potent acids, assisted by a boiling heat. It must be acknowledged therefore, that the combination still exists: and even that it is more intimate, since it is this that constitutes bodies the most homogeneous, most solid, hardest, and most capable of resisting fusion and solution.

[illegible]

According to Mr. d'Artigues, devitrified glass becomes fusible again, when, by reducing it to powder, the matter which had been separated, and which serve reciprocally as fluxes to each other, are again brought into contact. I conceived it was a proper subject for experiment to decide, whether this fusion would restore the glass to its former transparency, and other characteristic properties.

Experiment.

I took a piece of the glass No. 1 of Mr. Cifflé (p. 59), that is, window glass devitrified without any cement, remaining white, perfectly opake, and of extraordinary strength notwithstanding its thinness. After having reduced it to powder, I put 7 gr. [108 grs] into a platina crucible with a cover, and raised the fire to 160° Wedg. The result was a mass tolerably well fused, but white, inclining slightly to greenish, and having barely some appearance of being translucid on the edges; very smooth at the surface, but beneath it full of little cavities occasioned by the ebullition. There was a loss of weight of 59 mill. [0·9 of a gr.] or a little more than 8 thousandths.

Experiment with plate glass.

It became an interesting inquiry, to find what change a refusion would produce in plate glass, in which the mutual saturation of the silex and its fluxes is commonly more accurate; and particularly whether in this also there would be a diminution of weight. Into a platina crucible I put 62 gr. [957·6 grs] of pulverized St. Gobin plate glass, and kept it for 3½ hours at a heat of 48° Wedg. The result was a mass completely fused, the frizzled surface of which [*surface ratinée*], to use the term of the glass-men, indicated a slight commencement of devitrification*, which had a yellowish tinge, and somewhat greater hardness than the interior; alterations that Mr. d'Astigues had already observed in glass, which, being more simple in its composition, and more perfectly combined, resists the continued action of heat much more. The great number of blebs, that had formed in the lower part, did not allow me to determine with accuracy its specific gravity; but there was a loss of weight of 2 dec. [3 grs], or a little more than 3 thousandths, without any circumstance of the process giving room for the slightest suspicion, that it could have been occasioned by any thing but the loss of this quantity of matter*.

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* This surface, examined with a lens, exhibits an immense quantity of small fissures, forming by their intersections prisms with unequal sides. By causing the light to pass through the two opposite fractures on the sides, rudiments of crystallization are perceptible beneath the superior crust, which also indicate the first effects of devitrification.

To these strong reasons for rejecting the hypothesis either of a simple modification of structure, or of a precipitation of a portion of the component parts, let us add the increase of *hardness*, and *diminution of bulk*.

Farther argument from the hardness,

Among the products of devitrification, which I have laid before the class, there are several, as I have remarked, that cannot be scratched by rock crystal; there are some, on which corundum scarcely leaves a mark visible by a lens; and Mr. Ciffle's No. 5 scratches rock crystal, as an aqua marina would.

and density.

The density, that glass acquires in this process, is still more striking; though, like the hardness, it is only the effect of a more powerful aggregation. All the pieces, the specific gravity of which before and after the process I had an opportunity of comparing, gave a difference of 16 or 18 thousandths in addition.

Diminution of bulk in devitrified bottle glass.

Mr. d'Arcet had two cubes of bottle glass, of the manufactory of la Garre, cut, for the purpose of ascertaining their bulk, by the scale of Wedgwood's pyrometer, before and after devitrification. One advanced 17°, the other 11°; which gave for the first the proportion of 1000 to 909; for the second, 1000 to 952. The glass having been cut from the same piece, and consequently being of the same quality, the difference between the results can be ascribed only to a greater or less portion of the cement adhering to the surface of these cubes; which, however, was far from compensating for the diminution of the original bulk. This is farther proved by the colour, which the pieces devitrified in Reaumur's [illegible] assume at their surface; a colour, that often penetrates them to some depth, and can arise only from particles of metallic oxides contained in the sulphate of lime employed.

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* Mr. d'Arcet has sometimes found the weight of the cubes of glass, which he subjected to the process of devitrification, increased 5 thousandths of a gramme on 2 grammes [31 grs]; but he operated in a cement. The same must have been the case with the pieces No. 3 and 4 of Mr Ciffle, which, as has been seen, came out with a coating. But this proves nothing against the two experiments related above, made without cement, and in platina crucibles.

I think then I may conclude, that the characters and properties, by which transparent is distinguished from devitrified glass, are not solely the effect of crystallization, either of the same integrant particles, or of some of its elements, which would form a new compound, the others being separated by precipitation; but that there is really a change of proportions in the compound, by the volatilization of a certain part. It is not when the progress of chemical analysis daily teaches us, that less than a thousandth part of its substance added, or substracted from a compound, produces striking changes in its properties, that we can admit the explanation of so many characters, and of such striking properties, simply by the mode of structure.

In devitrification there is

a change of proportions by volatilization of some part.

XI.

Analysis of Olefiant Gas: *By Mr.* THEODORE DE SAUSSURE*.

THE inflammable gasses produced by the decomposition of vegetable substances were long considered as simple compounds of hidrogen and carbon: but, when the proportions of these elements were endeavoured to be ascertained by the quantity of carbonic acid gas produced in their combustion, and that of oxigen gas employed to burn them; it was found, that more water was formed, than ought to have resulted from the oxigen used: whence it was necessarily admitted, that the inflammable gas must have furnished the oxigen for this surplus of water. Mr. Berthollet has made the greatest number of experiments on this subject: he has subjected to analysis several inflammable gasses, obtained from the distillation of moistened charcoal, of oil, and of camphor; and he has found, that these gasses, all of which had been considered as compounds of hidrogen and carbon, and had been termed carburetted hidrogens, contain oxigen also, and should be

Carburetted hidrogen gasses presumed to contain oxigen.

Berthollet's experiments.

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* Ann. de Chim. vol. LXXVIII, p. 57. Read to the Society of Physics and Natural History at Geneva, April, 1810.

called oxicarburetted hidrogens. The gasses, which I obtained by the decomposition of alcohol, and of sulphuric ether, in a redhot tube, were found, after accurate analyses, to be included in the same class[*]; and it was the same with respect to the hidrogen gas, that Dr. Thomson obtained from the distillation of peat[†].

But olefiant gas not thoroughly examined.

After so many experiments concurring to prove, that oxigen is an essential intermedium in the aeriform combination of hidrogen and carbon, the examination of a farther number of compound inflammable gasses would seem superfluous; and it is no doubt for this reason, that the olefiant gas obtained by subjecting to a gentle heat a mixture of alcohol with four times its weight of sulphuric acid, has not yet been accurately analysed. This gas, discovered by the Dutch chemists, is particularly distinguished from every other inflammable gas, as is well known, by forming an oil, when mixed with oximuriatic acid gas, and by furnishing more light, and more carbonic acid, when burned.

Obstacle to its analysis.

When olefiant gas is detonated with the proper proportion of oxigen gas for burning it completely, it breaks the strongest eudiometers. This has prevented Mr. Berthollet from adding its analysis to those I have mentioned above: and he made no attempt to surmount this difficulty, because it might be presumed, that the olefiant gas contained oxigen, from an experiment of the Dutch chemists; who announced, that this inflammable gas, if passed through a redhot porcelain tube, expanded, and acquired all the properties of the oxicarburetted hidrogen obtained from sulphuric ether by the same process[‡].

Presumed to contain oxigen.

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[*] See Journal, vol. XXI, p. 326.

[†] Ib. vol. XVI, p. 241.

[‡] When the Dutch chemists mentioned these facts, they were unacquainted with the methods since invented for analysing inflammable gasses with accuracy; and consequently could not make this comparison with precision. It is probable, from other experiments, that carbon must have been deposited in the tube. This process requires many precautions, for us to place any confidence in its results. It is necessary, that the olefiant gas should be perfectly dry, and have no oxigen from the atmosphere mixed with it.

Dr. Henry

Dr. Henry has made known the gaseous products of the slow combustion of the olefiant gas in an apparatus of his invention, which exposes the operator to no danger from the breaking of the vessels*; but from his results he has not deduced the consequences, that might be drawn, respecting the analysis of this gas. I shall exhibit them here, following the data of the English chemist.

* * * * * * * * * * * *†

The author's experiment.

I proceed to the results, which I have obtained in repeating all these experiments.

Mode of preparing the gas.

The olefiant gas was prepared by mixing pure alcohol with sulphuric acid in the proportions mentioned above. I interrupted the distillation before the white vapours, produced by the presence of sulphurous acid, were abundant. This sulphurous acid, which was in part in the state of gas, was absorbed by liquid ammonia.

When I weighed the olefiant gas, the barometer was at 0·71893 [28·28 in.]. The thermometer attached to the barometer was at 3·75° [38·75° F.], and the temperature of the gas was the same.

The capacity of the receiver was 3527·8 cubic cent. [214·83 cub. in.].

The mercurial gage, in the receiver exhausted of air, stood at 3·5 mil. [1·38 line]. The difference of weight between the empty receiver, and the receiver filled with the olefiant gas at the extreme of moisture was 4·147 gr. [64·068 grs] without any correction.

The difference of weight of the receiver when empty and when full of atmospheric air, under the same circumstances, was 4·21 gr. [65·026 grs].

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* Bibl. Brit. vol. XLI, p. 324 [See Phil. Trans. for 1808, p. 282; or Journal, vol. XXII, p. 83, for the original.]

† On comparing what follows here with Dr. Henry's Paper, it appears, that the French translator has made several mistakes with regard to the figures, by which Mr. de Saussure has been misled. What he says therefore does not apply; and, as of course it would be useless, it is omitted. C.

Hence

The specific gravity.

Hence it follows, that the weight of dry atmospheric air is to that of dry olefiant gas as 1000 to 985·2.

I have found by direct experiments at the temperature of 12·5° [54° F.], that the litre [2·1 wine pints] of dry atmospheric air at 0·76 of a met. [29·8 in.] weighs 1·228 gr. [18·967 grs]; consequently, from the ratio I have given above, the litre of dry olefiant gas weighs 1·2098 gr. [18·686 grs] at the same pressure and temperature.

Analysis in the eudiometer.

I now proceed to the analysis of the gas by its combustion over mercury in Volta's eudiometer. I have already said, that this instrument bursts, when olefiant gas is detonated in it, with nearly the proportion of oxigen gas requisite to burn it: but I prevented this accident, by employing a much larger proportion of oxigen than the olefiant gas could consume.

Mode of executing it safely.

Result.

I mixed 100 parts of the latter with 500 parts of oxigen gas deprived of carbonic acid by potash. These 500 parts of oxigen contained 23·5 of nitrogen, and 476·5 of pure oxigen. The mixture was reduced by detonation to 409·5 parts. Potash and hidrosulphuret of potash demonstrated in this residuum 201 parts of carbonic acid gas, 184·5 of oxigen gas, and 24 of nitrogen.

The combustion nearly complete.

After the separation of the carbonic acid in the residuum of the detonation of the olefiant gas, I examined whether the whole of the olefiant gas were burned, by adding to the residuum a small portion of hidrogen gas, measured with great precision, and detonating the mixture. By this second detonation not more than one hundredth of carbonic acid at the most was formed: the condensation of the gasses by the combustion was equal within a hundredth to what should have resulted from the action of the hidrogen gas I had added. The first detonation therefore had effected the combustion of the olefiant gas. In the calculations from this analysis I have paid no regard to the products of the last operation, because they are scarely to be distinguished from errours of observation.

Component parts of the gas.

From these experiments it follows, that 100 parts of this olefiant gas consumed for their combustion nearly 292 parts of oxigen gas to form water and 201 parts of carbonic acid. By comparing these numbers with the litre, and substituting the corresponding weight, we find, that 100 parts by weight of dry olefiant gas contain

Carbon	84·78
Hidrogen	13·55
	98·33

The sum of these products represents very nearly the weight of the olefiant gas, that I had subjected to analysis: the difference of a hundredth and half may be ascribed to errour of observation, or indeed to the small quantity of inflammable gas, that escaped combustion. Hence it follows, that the olefiant gas does not contain any observable quantity of oxigen, that it is composed of hidrogen and carbon, and that it should be termed *carburetted hidrogen.*

It contains no oxigen.

Olefiant gas appeared to me to vary a little in its weight and composition, according to the mode in which it was prepared. When the distillation of the alcohol and sulphuric acid is carried too far, the gas obtained after the separation of the sulphurous acid is a trifle lighter, and contains a little oxigen. In my experiments, however, this oxigen never exceeded the four-hundredth part of the gas.

Varies a little according to the mode of preparation.

In the processes which I conducted so, that there was no oxigen in the olefiant gas, I did not always find it precisely of the same gravity, or with the same proportion of hidrogen and carbon; but the difference amounted only to two or three hundredths, and consequently was not altogether independent of errours of observation. In the experiments, in which I obtained olefiant gas of the greatest specific gravity and most loaded with carbon, its weight was precisely that of atmospheric air. The litre [2·1 pints] of this dry gas weighed 1·228 gr, [18·967grs], at 0·76 [29·8 in.] pressure, and 12·5° [54·5° F.] temperature, 100 parts of this gas, mixed with 500 of oxigen, were reduced by detonation to 402; which contained 208 of acid gas, and 194 of oxigen; not mentioning the 12 parts of nitrogen mixed with the oxigen, which were found, to about a hundredth, in the residuum of the process. 100 parts of olefiant gas by weight therefore consumed 306 parts of oxigen gas, and formed

The difference trifling, when not contaminated with oxigen.

Heaviest equal to atmospheric air.

formed 208 of carbonic acid. Hence it follows, that 100 parts of olefiant gas by weight contain

Its component parts.

Carbon	86·13
Hidrogen	14·64
	100·77.

General conclusions.

From all these analyses I conclude, that olefiant gas when properly prepared, contains no sensible quantity of oxigen. In this state its specific gravity is equal or but little inferior to that of atmospheric air. One part of this gas by bulk consumes nearly three of oxigen for its combustion, and forms two parts of carbonic acid.

Omitting fractions, olefiant gas contains, by weight,

Carbon	86
Hidrogen	14
	100.

Condensation of [illegible]

Fifteen parts of hidrogen appear to suffer a condensation to about half their bulk in dissolving 85 parts of carbon; and the olefiant gas thence resulting has by calculation very nearly the specific gravity, that I found in my first experiment.

XII.

Abstract of a Paper on the mutual Action of metallic Oxides and alkaline Hydrosulphurets. by Mr. GAY-LUSSAC*.

Mutual action of metallic oxides, and alkaline hydrosulphurets.

THE paper, to a sketch of which I here confine myself, includes the experiments I have made on the mutual action of metallic oxides and alkaline hydrosulphurets. I found,

1st, That the metallic oxides, in which oxigen is greatly condensed, as those of zinc and iron, do not decompose the hydrosulphurets.

2dly, That all the other oxides decompose the hydrosulphurets, and yield products, some of which vary according to the particular nature of the oxides.

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* Ann. de Chim. vol LXXVIII, p. 86. The principal results were mentioned in a chemical lecture at the Polytechnic School on the 10th of April, 1811.

3dly, That sulphuric acid is never formed.

4thly, That there are constantly formed water, and sulphites or sulphuretted sulphites; and frequently metallic sulphurets.

5thly, That it is consequently impossible, to obtain the bases of the hydrosulphurets pure by means of metallic oxides.

6thly, That, when a sulphuret is dissolved in water, there is never any sulphate formed, as was generally supposed, but sulphites, or sulphuretted sulphites.

Experiments

I shall relate some of the experiments, that led me to these results; and take first as an example the black oxide of manganese, and very pure and colourless hydrosulphuret of potash.

with oxide of manganese and hydrosulphuret of potash.

As soon as these two substances are mixed, their mutual action announces itself by a very sensible elevation of temperature; the hydrosulphuret takes an orange yellow colour, like the sulphuretted hydrosulphurets; and, when muriatic acid is poured in, sulphur is precipitated, and sulphuretted hidrogen evolved. On heating the mixture, it speedily loses its colour, and becomes as limpid as water. At this point the liquid, which is strongly alkaline, precipitates acetate of lead of a white colour; and it might be supposed, that it contained only potash: but, if muriatic acid be poured in, it immediately becomes turbid, sulphur is thrown down, and sulphurous acid gas is evolved. If, after having boiled and filtered, muriate of barytes be added, no precipitation takes place. Lastly weak sulphuric acid, poured on well washed oxide of manganese, dissolves cold a large quantity, without the evolution of any gas, particularly of sulphuretted hidrogen.

Hence it follows,

Results.

1st, That the first effect of the oxide on the hydrosulphuret is to convert it to the state of a sulphuretted hydrosulphuret, acting in this as the air does on hydrosulphurets, and very probably giving rise to a sulphuretted sulphite from the commencement of the process.

2dly, That a great deal of sulphuretted sulphite is afterward formed.

3dly, That no sulphuric acid is produced.

4thly, That the black oxide of manganese is brought back to a minimum, and that no sulphuret of manganese is formed.

Oxide of copper and sulphuretted hydrosulphuret of barytes.

As a second example I shall take the brown oxide of copper, and the sulphuretted hydrosulphuret of barytes. These two substances act strongly on each other; and, if they be heated, the liquor presently loses its colour, and no longer contains any thing but barytes mixed with more or less sulphuretted sulphite. The oxide, after having been washed till the water that comes off is no longer precipitated by sulphuric acid, effervesces with muriatic acid in consequence of the sulphurous acid evolved, and a great deal of muriate of barytes is formed. The residuum, washed anew to remove the latter salt, and then treated with very weak nitromuriatic acid, leaves no other residuum than sulphur, which collects on the surface of the liquid.

The two experiments compared.

Hence we see, that the oxide of manganese and the oxide of copper, though exhibiting the same general result, have acted differently in this, that no sulphuret of manganese was formed, though sulphuret of copper was produced: but the cause of this is, the oxide of manganese was only reduced to a minimum, and in this state it has little affinity to sulphur.

Solution of sulphurets in water.

I shall not relate any more experiments of this kind, but shall conclude with a brief account of what happens, when a sulphuret is dissolved in water.

Sulphuret of barytes.

I made some sulphuret of barytes and sulphuret of potash with a gentle heat. The first, dissolved in water, left a residuum, which, after having been washed, dissolved completely in muriatic acid, evolving a great deal of sulphurous acid.

Sulphuret of potash

The solution of sulphuret of potash, into which I poured muriate of barytes, yielded but a slight precipitate, which dissolved completely in muriatic acid. The mixture had been heated, and on cooling a great many little crystals of sulphuretted sulphite of barytes were deposited on the sides of the vessel.

Sulphuretted sulphites.

I found also, that the sulphuretted sulphites were not altered by exposure to the air; and that a neutral sulphite can dissolve a great deal of sulphur, without becoming acid or alkaline.

XIII.

XIII.

On the Ore of Platina of St. Domingo: by Mr. GUYTON-MORVEAU*.

Platina reported to have been found in St Domingo some years ago.

IT had long been supposed, that platina was found only in the gold mines of Santa-Fe and of Choco in Peru. Twenty years ago there was a report, that some had been obtained from a ferruginous sand in St. Domingo; but apparently the examination of this was not executed so as to give decisive results, since it has not been published. The report, no doubt premature, of its existence in Siberia, has likewise died away. The singularity of such a limited and apparently exclusive situation remained attached to platina, till Mr. Vauquelin found as far as 10 per cent of it in the gray silver ore of Guadalcanal; where he has no doubt it is in the metallic state, but without being accompanied with either of the four metals lately discovered in the platina ore of Peru†.

The lately received.

What Mr. Percy submitted to the inspection of the Institute, on the 12th of Feb. 1810, leaves no doubt of the existence of this metal in the eastern part of St. Domingo. It was brought thence by surgeon-major Dubizy, an enlightened naturalist. It exhibits precisely the same characters as that we have from Spain. The grains, equally flattened, are in general a little larger, and its specific gravity is a little greater; which may arise from its having been more carefully freed from foreign matter, though the magnet still separates some from it. It is found, principally after heavy rains, in the sands of the river Yaqui, at the foot of the mountains of Sibo. These sands, which contain also a little gold, are collected by women, who, without washing them, sell them for a few maravedis.

Some gold with it.

Mr. Jeannety, who has begun to manufacture a few hectogrammes of this ore, informs me, that, having slightly calcined it, and afterward poured on it sulphuric acid, he perceived a few grains of gold among it.

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* Ann. de Chim. vol. LXXIII, p. 334.

† Ann. de Chim. vol LX, p 317; or Journal, vol. XVII, p 129

‡ Mr. G M should also have mentioned the ore from Brazil of which an account is given by Dr Wollaston, in the Phil Trans for 1809, p 189. See Journal, vol XXV, p 18 C]

SCIENTIFIC

Wernerian Society.

Two species of arctic gull.

AT the first winter meeting of this society, an interesting communication from Dr. Arthur Edmondstone was read, concerning the larus parasiticus, or arctic gull. Owing to the remote situation of the haunts of this gull, its history and manners have hitherto been little known. Dr. Edmondstone has now illustrated them. He has observed two kinds of arctic gulls in the Shetland Islands, the common sort, with the breast and belly of a mouse colour, and another sort, with the breast and belly pure white. Each kind keeps together, and the white is a larger and heavier bird, but less bold than the other. The doctor is therefore inclined to consider these not merely as varieties, but as distinct species.

Varieties of zircon in Scotland, and subspecies of rutilite.

At the same meeting professor Jameson read to the society a short description of several varieties of the precious stone named zircon, which he had lately discovered, imbedded in sienite, in Galloway. He also informed the society, that he had observed, in the same rock in Galloway, both the brown and the yellow subspecies of that very rare ore known to mineralogists by the name of rutilite, or sphene.

Mr. Edgeworth's new invented spire.

Mr. Edgeworth informs me, that an iron skeleton of a spire, agreeably to his construction described, vol. XXX, p. 241, may be covered with thin *flags* of Portland stone or with any other thin flags or stones, that do not imbibe water, and that are of a pleasing colour; that the Board of First Fruits in Ireland, which consists of all the archbishops and bishops, thought proper, without any solicitation, to present the parish of Edgeworthstown with the cost of the spire; and that the Dublin Society have ordered a working model to be made of the spire, and the machinery employed in raising it, to facilitate the erection of such ornamental buildings in different parts of this country.

The views of the coast of Ireland from the Bay of Dublin are uncommonly beautiful, but the city appears flat and uninteresting from its having scarcely any elevated building, the only spire in the whole city being that of St. Patrick's church. As the cause of this defect has probably been the expense attending the construction of steeples in the usual mode, we may now hope, that it will not long continue.

The

Information requested respecting the writers of the mathematical questions and their answers in the Ladies Diary.

The annual publication called the *Ladies Diary* or *Woman's Almanack*, has every year, for upwards of a century, contained a certain number of mathematical problems, to be answered in the Diary of the following year. The publication of these has answered several valuable purposes; in particular it has awakened the attention of many to the study of the mathematical sciences, who would not otherwise have thought of them: the questions have served to exercise the ingenuity, and call forth the exertions of young mathematicians, some of whom have in time arrived at great eminence, as cultivators of mathematical learning: and, lastly, the work has served as a repository for the preservation of many curious mathematical disquisitions, which, but for this mode of publication, would never have been known to the world.

The beneficial influence, which the Lady's Diary has exerted upon the state of mathematical science in this country, has been long felt and acknowledged; and has been particularly noticed by the writer of that very valuable analysis of the Mécanique céleste, given in the Edinburgh Review. Speaking of the comparative state of mathematical knowledge in England and on the Continent, he says: "A certain degree of mathematical science, and indeed no inconsiderable degree, is perhaps more widely diffused in England than in any other country in the world. The *Ladies Diary*, with several other periodical and popular publications, are the best proofs of this assertion. In these many curious problems, not of the highest order indeed, but still having a considerable degree of difficulty, and far beyond the mere elements of science, are often to be met with. And the great number of ingenious men, who take a share in proposing and answering these questions, whom one has never heard of any where else, is not a little surprising. Nothing of the same kind we believe is to be found in any other country.—The geometrical part has always been conducted in a superior style; the problems proposed have tended to awaken curiosity, and the solutions to convey instruction in a much better manner, than is always to be found in more splendid publications."—(See Edin. Rev. vol. XI, p. 282.

Information requested respecting the writers of the mathematical questions and their answers in the Ladies Diary.

A collection of all the mathematical questions, as well as other parts of the Diary, from its beginning to the year 1772, was published about that period, by its present ingenious and learned editor, Dr. C. Hutton, late of the Royal Academy, Woolwich. That work however being now out of print, and the stock of questions now considerably increased, Mr. T. Leybourn, editor of the Mathematical Repository, has issued proposals for publishing by subscription all the mathematical questions, and their answers, from the commencement of the Diary to the present time. Beside the valuable notes given in Dr. Hutton's edition, the present editor intends to give others, and in particular, he means to give, as far as he can, brief notices of any circumstances he may be able to learn respecting such authors of the answers to the questions as are dead, and even of such as are alive, when it can be done with propriety.

But as many of the authors have now been dead for a number of years, and have not been known beyond the particular circle of their friends, he is aware, that this part of the work can only be rendered tolerably complete by the assistance of such friends to his undertaking, as may be capable of giving the information here specified. He ventures therefore, through the medium of the Philosophical Journal, to solicit communications respecting the authors of the mathematical parts of the Diary. These may be addressed to him at the Royal Military College, Great Marlow, Bucks.

London Hospital.

Medical lectures.

Dr. Buxton's Spring Course of Lectures on the Practice of Medicine, will be commenced about the 20th of Jan. 1812.

Anatomical Theatre, Bristol.

Chirurgical lectures.

Mr. Thomas Shute will commence his spring course of Lectures on Anatomy, Physiology, and the Principles and Operations of Surgery, on the 17th of January, at eight in the morning.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1812.

ARTICLE I.

On the Mechanism of Flowers. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I AM now to give the mechanical management of the flowers.

Mechanism of flowers.

That each natural species has a different mechanism, one peculiarly suited to its shape and structure, I am perfectly convinced, from the dissections I have made of such numbers, having tried every sort of flower, I could procure. I am indeed not fortunately situate in this respect; as I am not near many gardens where curious exotics can be had, so that I am obliged to be contented, in a great measure, with what the indigenous soil and the common garden will yield, which do not produce me that variety I could wish, Indeed I did not intend to pick out the most animated flowers, lest it should be said, that they were exceptions to a general rule; I have rather taken the first that occurred, as there is no flower without mechanism, or the beautiful arrangement of which is not such as to be well worth the contemplation of a philosophic mind: and when it is considered what a quantity of plants there are, and that each species has its appropriate contrivance, what an idea does it give us of the Creator of all!

The mechanism of the polygalæ.

I shall begin with the mechanism of the polygalæ, all the species of which are, I believe, managed in the same way. To defend the stamen and pistil, till the seed is to be impregnated, a hood covers them; but as it is also necessary, that the sun should, at the same time, shine on the female, the hood has a gatherer behind, see Pl. III, fig. 1, *a a*, to raise or depress it. When the sun appears, the contraction of this gatherer, in which runs the spiral wire, draws back the hood as at fig. 2. *b b* is a small ball, on which it turns as on a swivel. When it falls back, the drops appear on the pistil, and are seen saturated with the powder of the stamen; but if a cloud should in the mean time obscure the sun, and the wind rise, the gatherer behind would directly be lengthened, the hood return to its place, and the drops of the pistil retire within the orifice.

How often have I watched this beautiful process! Seated on the ground I have at pleasure made the hood rise and fall by the help of the sun, and a wet napkin, alternately admitted over it; thus viewing all its mechanical wonders at once.

The flower dilates and contracts.

The wings, represented at fig. 2, 3, 4, have also a part which contracts and dilates, fig. 3, *d d*. The spiral wire, meeting at *e e*, fig. 4, is fastened to the exterior edge of the wing, passes thence to *c c*, where it forms knots, and is then collected and drawn together to a sort of pin, round which it winds, while the outside part, fig. 3, *d d*, gathers up like a gatherer, and throws the wing from it, or draws it near the body, alternately, as fine weather, or bad requires it; as the part *f f* will necessarily open it by the contraction of the spiral wire, and throw it from the body of the flower to admit the sun to the seeds, that they may be perfected by its rays.

On examining a flower this effect will be easily seen. The lengthening of the filaments also serves to keep the hood on, when the wind blows; but when the sun shines, they contract, and the hood is again at liberty to retire back. It seldom falls quite off. It is curious, that the species of swivel, which belongs to it, is common to plants in general; but is so contrived, that the part, within which it turns, is but three quarters of a circle, see fig. 5, so that if with the finger you turn the hood quite down, it comes off. The same piece of mechanism is foundin the phaseolus leaf, and in many flowers. It is inconceivable what a perfect weather glass this flower is: not a cloud can pass over the sun, that does not change its position; and every night the whole flower is drawn up close, and shelters itself under the leaves.

Mechanism of the phaseolus.

The next flower, which I shall consider, in some measure resembles the last, being a diadelphian, the phaseolus vulgaris. Here the principal contrivance is the contracting of each part to the form of the pistil, by the means of the spiral wire. The wings are moulded in the same model, so that by drawing the spiral wire tight, it must necessarily slip off the part on which it is placed; and by this means throw itself to a certain distance from the body of the flower. In its perfect state, fig. 6, *g g*, Pl. IV, is fastened on to *h h*, figs 7, 8: of course, when the top of *g g* is contracted it will push off *h h*, by which means the flower opens to the sun, and sends back the banner fig. 9; which contracting in its turn by the means of two plaits under the collar *i i*, it can wave itself backward and forward to admit the sun, or to shade the body of the flower from it. In the same manner the part *g g*, fig. 6, by contracting, fills it up, and, drawing the side which projects, almost turns the wings round, and throws the banner at a distance, leaving the pistil to twist till the keel *w*, figs 7 and 8, is broken, which exposes the female to the sun. Then the usual process takes place. I have observed, that, when the phaseolus grows in a hothouse, the keel generally breaks; but it appears, that the temperature is not high enough, to produce this effect in the open air in this country: we properly therefore can trust the indigenous flowers only, to teach us to understand the real motion of a flower. Nature certainly intended the keel to break, and expose the pistil to the sun's influence, while the drops appear on it: as the keel generally slips off from most of the diadelphian

Difference in a hothouse.

plants at that time. This female is also another proof of the spiral wire being inserted without a case, and therefore twisting much.

Effect of the frost and lightning.

How powerful is the mechanism of the antirrhinum! The strength of the spring is such, that it appears as if made of steel, rather than of a thin cuticle, with a pabulum of diminutive round bubbles of water. There is not any thing more wonderful than the strength in flowers, and in herbaceous or annual plants. I could scarcely believe, that it could proceed from their fulness alone, and that when I found a stem of cabbage as hard as wood, a frost could in one moment render it as soft as pap: and yet it is certainly so. Lightning and frost produce exactly the same effect on them. On examination of the stem of vegetables, after being thus struck, the vessels appear broken, and the liquid discharging itself. It is the same with a leaf or flower: the strength of the corolla depends wholly on the balls of water, which forms its pabulum, and the vessels, which are partly wood, partly spiral wire. Press them lightly between the finger and thumb, the bubbles are broken, and the whole becomes a soft wet rag. There is however a curious part of this process I cannot at all comprehend; it is the circular turn both frost and lightning produce in all the thick stems of herbaceous or annual plants. When thus struck, their parts are found not only broken, but twisted, so that the whole interior will be entirely misplaced, and all one way. But I should apologise for this digression.

Mechanism of antirrhinum.

The spring of the antirrhinum is formed by the thick wood vessels *r s*, fig. 10, turning within that which surrounds them; so that when the spiral wire draws these tight, they push against the springs, and the flower opens its mouth. But there are many other motions caused by the pressure of the filaments, contracting and pushing crossways against the opposite sides of the flower; while the bending of the fibres *n n*, from their interior contraction, alternately presses each stamen to the glutinous drop of the pistil, now ready for their reception. There is not a more beautiful arrangement than this, which may be seen by pressing the two sides so as a little (when the sun shines fully on it) to open the flower; but I doubt not there is in this part of the process some degree of electricity, which contributes: as it is only in a particular state of the pollen, that the stamen bends towards the pistil; it now does so but when the sun shines; and if you shade the flower, you stop the effect, at least prevent the other stamens proceeding in turn. The mechanism that makes it move toward the pistil is plain and evident: but that it should move only when the pollen is perfectly ripe, I must believe owing to some electric attraction between the liquor of the pistil and the flower of the stamen. About that time, if the antirrhinum is watched, it is in perpetual motion in the interior; and indeed this is the case with most flowers: but if one of the stamens is abortive, this remains totally still though on examination it has exactly the same mechanical formation, by which the others are impelled toward the pistil*. I shall, when the season returns, give the tulip, the violet, and the bianca, but I shall wait till they appear in summer, in hopes to tempt young people of my own sex to follow me in the examination. There are many flowers, the mechanism of which is far more curious than that of those already described. But the contrivance evinced in the formation of the stamens and pistils far exceeds that in the corolla. The pistil of the violet has a regular trap to catch insects. The drop appears within a cup, when the juice is ripe; but if any fly attempts to place its proboscis within this cup to plunder it, a bag draws up, and closes the entrance in so quick a manner, that the poor insect within breaks off the part caught, or dies in consequence. But as it is only at the time of the impregnation of the seeds, that the drop appears, few are the victims of this mechanism. It is a great mistake to suppose, that there are only a few flowers so formed.—There is hardly any one, which has not some contrivance to protect the chief part of the honey, to secure the impregnation of the seed. Nature ordains, that the insect tribe should take that which remains generally at the bottom

Perpetual motion in the interior.

Mechanism of the pistil.

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* Is not this a sufficient proof, that, though mechanism is necessary to the motion of plants, the *cause* of this motion is something very different from mechanism? C.

bottom of the cup of the corolla; but if it even permitted them to steal also the drop it shows, which attracts the flower of the stamens, the seeds would seldom be impregnated.

Mimosa sensitiva. I by no means think, that the spiral wire being, or not being, the cause of motion in plants, is decided by our completely understanding the management of the mimosa sensitiva. I should as soon think, that steam being [illegible] governing principle of the steam-engine depended [illegible] perfectly comprehending the conduct of the [illegible] is to the complete organization of plants in general [illegible] where their mechanism is found far more simple, and [illegible] proved: not to a plant, the formation of which differs so entirely from every other, as to require the most excessive nicety in weighing the different powers against each other. That it is a mechanical object its formation alone proves. The rest, when I better understand this part of botany, I shall hope more plainly to show.

I am, Sir,
Your humble servant,
AGNES IBBETSON.

I shall add a short description of the parts of the flower:

Plate III, fig. 1—5, the common milkwort.

Fig. 2 shows the cap thrown back, to admit the sun's rays.

Fig. 3 the wing, separated from fig. 1, to which in its natural state it is fastened at *x x*.

Fig. 4, the same dissected, showing the manner in which the spiral wire is collected from the exterior of the wing, and conveyed thence to the body of the flower by means of the pieces *e e*, *f f*.

Fig. 5, the ball, or swivel, on which the hood turns, as shown at *b*, figs 1 and 2.

Plate IV, fig. 6, is the wing of the phaseolus vulgaris; fig. 9 the banner: fig. 8 the pistil, clothed with the keel; and fig. 7, the same without it. *w* is the part which breaks, to set at liberty the pistil in the interior. Though it will perform its functions without this, yet I should suppose not so well.

Figs 10, 11, the antirrhinum divided into two parts.

Mechanism of Flowers.

Fig. 1.

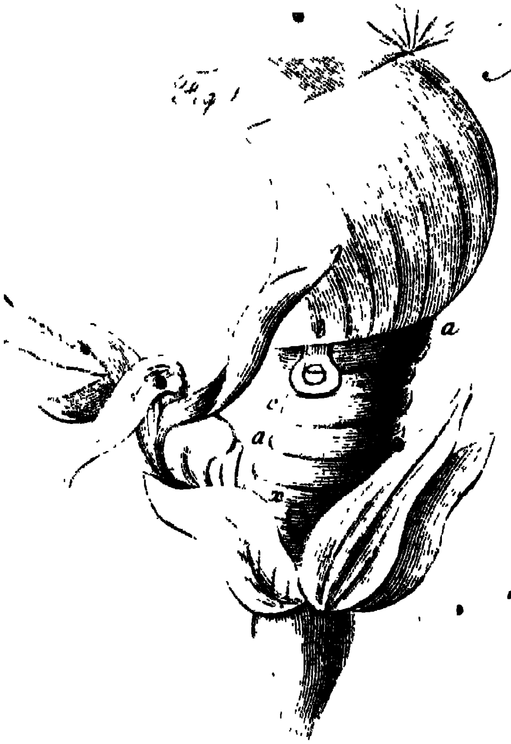


Fig. 2.

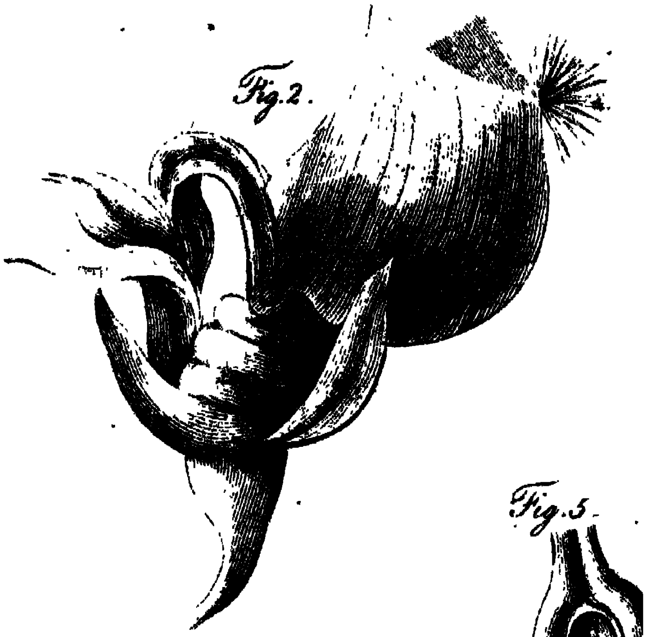


Fig. 5.

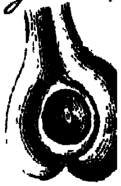


Fig. 3.

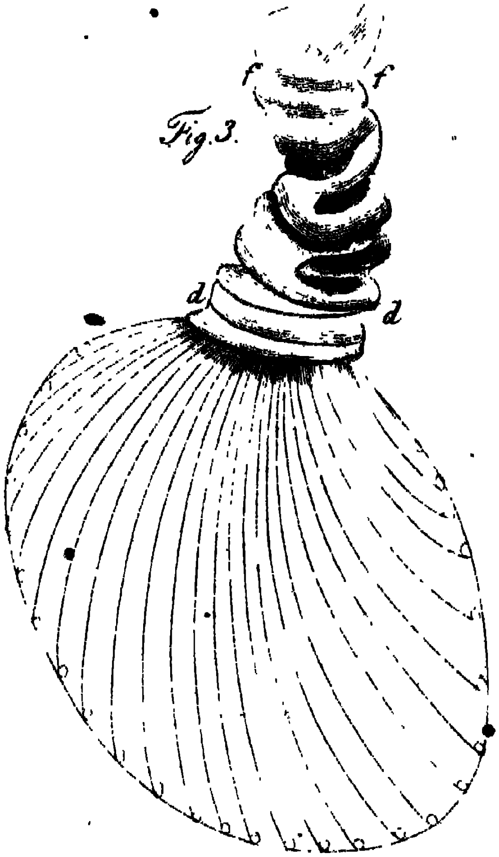
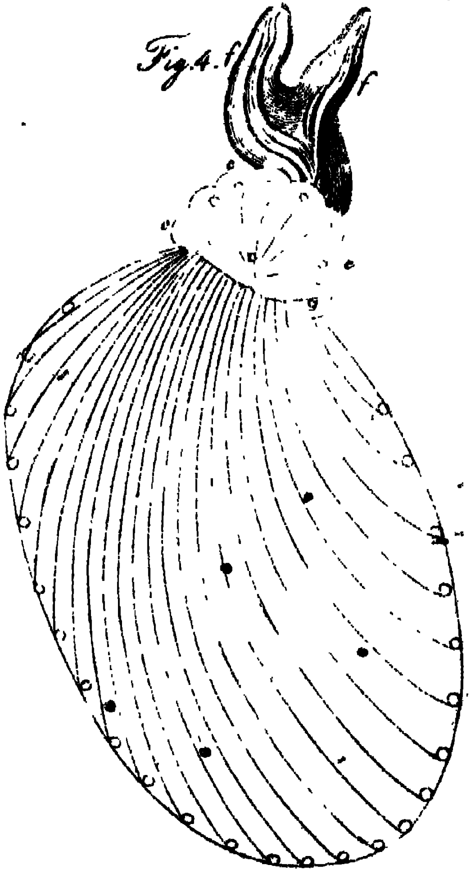


Fig. 4.



Mechanism of Flowers.

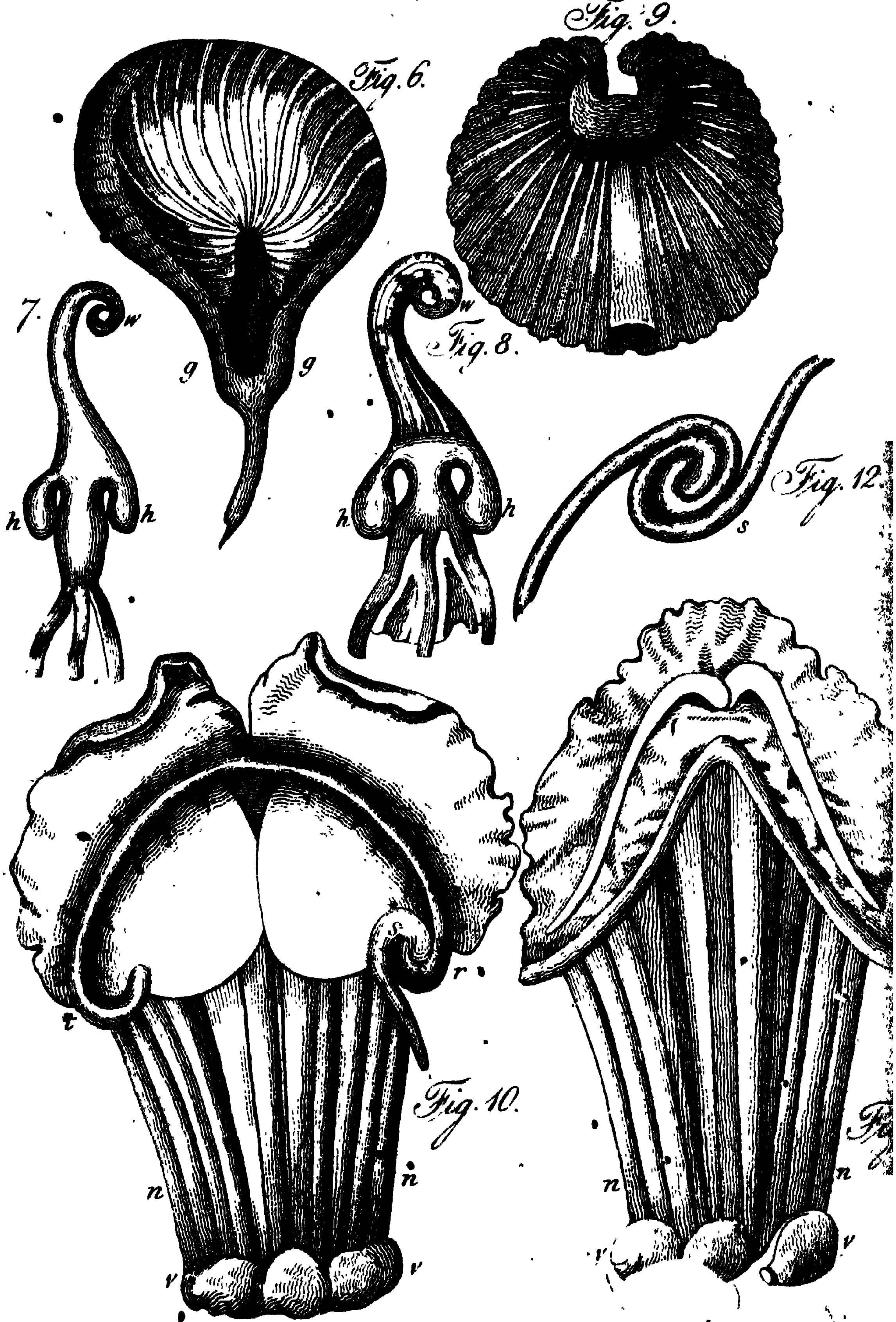


Fig. 12, *rs*, shows the spring, by which the flower is opened and shut, as it is when twisted. By pressing *t r*, fig. 10, the flower is opened; *v v* are the nectaries, which increase the strength of the flower. I shall give the stamens and pistil, when I show the mechanism of the parts, which will prove how much the wood vessels *n n* contribute to the management of the flower by the interior drawing of the spiral wire.

II.

Remarks on Mr. ANDERSON'S *Experiments "On the Decomposition of Water in two or more separate Vessels", with an Account of Mr.* MURRAY'S *Experiments on the same Subject.*

To W. NICHOLSON, Esq.

SIR,

Inferences from the decomposition of water in separate vessels:

IN the number of your Journal for November I have read with much pleasure some experiments on the "decomposition of water, in two or more separate vessels," by Mr. Anderson of Perth. They were professedly made to correct an inference drawn from an experiment of Ritter, the result of which led some persons to suppose, that the elements of water could be transmitted, in opposite currents, through the substance of a metallic wire; while others, unwilling to admit the permeability of metallic matter by gaseous bodies, were disposed, from this experiment, to doubt altogether the commonly received opinion with respect to the compound nature of water.

Both oxigen and hidrogen found in each.

Mr. Anderson first repeated the experiment of Ritter, and found, that water was decomposed in the manner that had been stated; and employing afterward an apparatus, in which all the gaseous products could be collected, he likewise ascertained, that they consisted not of oxigen only in one receiver, and of hidrogen in the other, as Ritter had supposed; "but these two substances were found in each receiver, in the exact proportion in which they combine together to form water". Consequently, water had been decomposed in each receiver, and no transmission of its elements, in opposite currents, through the connecting wire, can be supposed to have happened: neither is there any ground for considering water as a simple body, but a compound of the two elementary substances above stated.

The fallacy of Ritter's statement already shown by Mr. Murray.

By these experiments, then, Mr. Anderson has succeeded in detecting the fallacy in Ritter's experiment, and the consequent errours in the conclusion drawn from it; but, from not being acquainted with what has been done by others, he is not correct in the observation, that "no person "appears to have suspected the accuracy of Ritter's state-"ment, or even to have repeated his experiments with any "degree of care". The truth is, this philosopher's experiment was repeated more than twelve months ago by Mr. Murray, and the fallacy of his statement completely detected.

His account of it.

In his Elements of Chemistry, published in October, 1810, Mr. Murray (vol. I, p. 308) refers to this experiment of Ritter, in which, says he, "it is stated, that when "a wire attached to the positive side of a galvanic battery is "placed in water in a tube, and a wire from the negative side "is placed in another portion of water in another tube; and "when these are connected, not by placing them in a vessel "of water, but in separate vessels connected by a metallic "wire; the usual pheuomena are produced, and the oxigen "is evolved at one wire, and the hidrogen at the other". Mr. Murray then goes on to state, that Ritter, conceiving it impossible, that the elements of water could be conveyed through the metallic wire, was led to conclude, that "the "communication merely of positive and negative electricity "to water caused it to assume these gaseous forms. Were "the fact as it is stated," continues Mr. Murray, "the "conclusion would perhaps follow. I have found, how-"ever, that it is a mere deception. The connecting wire "becomes a galvanic one, and its two extremities becom-"ing electrical, by what electricians have denominated "*position* or *induction*, are in states of electricity the re-"verse of the galvanic wires in the tubes; and hence oxi-"gen and hidrogen are evolved at their extremities, corre-"sponding with the hidrogen and oxigen evolved at the

" others; the extremity of the connecting wire, for example, " in the tube, in which the positive galvanic wire is inserted, being negative, and its other extremity, in the " tube in which the negative wire is inserted, being *positive*, " and therefore giving off oxigen corresponding to the " hidrogen, which appears at that wire".

Mentioned also by Mr. Ellis.

These facts, ascertained by Mr. Murray, have likewise been mentioned in a late work of Mr. Ellis*, who gives the detail of another experiment, made by Mr. Murray, in which the phenomena were rendered very obvious and striking. " The wires of the battery were made to pass through " glass tubes, and the tubes were then placed in two " glasses, which were connected by the metallic arc. Instead of water, however, both the tubes and glasses were " filled with an infusion of red cabbage, which held a " neutral salt in solution. As soon as the electricity was " put in motion, the neutral salt, in each tube and glass, " was decomposed; and the effects were at once conspicuous " on the vegetable infusion. For on the side connected " with the *positive* end of the battery, the fluid in the " tube was reddened, while, in the glass of the same side " it was rendered green. On the contrary, the fluid in the " tube connected with the negative side was green, and in " the glass of the same side it was red. Hence decomposition had taken place on each side: and while the positive " pole of the battery attracted, as usual, the acid which " reddened the infusion in the tube of that side, the negative extremity of the arc attracted the alkali in the glass " below, and changed its fluid to a green: and by the opposite electricities of the respective wires, reverse effects " were produced in the fluids of the tube and glass connected with the negative side of the battery".

Another experiment of Mr. Murray's.

Mr. Anderson's experiments confirm these.

Although it appears, that Mr. Murray had not only suspected, but actually detected, the fallacy in Ritter's experiment, long before the publication of Mr. Anderson's essay, yet I am far from insinuating, that the latter gentleman was at all acquainted with what the former had done. Indeed the train of thought, which seems to have suggested the experiments of Mr. Anderson, the methods which he followed, and the explanations which he has subjoined, all bear witness to the originality of his views; while the manner in which his experiments are executed and detailed is alike creditable to his ingenuity and skill. Still it is right, that the merit of *priority* should be bestowed where it is justly due; and it is this consideration alone, that has induced me to make the present communication. I will add, that I do not at all regret, in this instance, the circumstance of Mr. Anderson's being unacquainted with Mr. Murray's experiments, since it has prompted him to institute inquiries, which he has shown himself so well able to conduct, and the results of which so satisfactorily confirm the conclusion, at which Mr. Murray had arrived.

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* Farther Inquiries concerning Vegetation, &c., p. 181.

The theory of induction renders the supposition of any passage of the electric fluid unnecessary.

I shall conclude by observing, that the electrical law of *induction*, which Mr. Murray has pointed out, as affording an explanation of the *manner* in which these decompositions are effected, renders it unnecessary to resort to the supposition of a conveyance of electric matter, in opposite currents, through the water and the wire, in the way which Mr. Anderson has suggested.

I am, sir,

Your very obedient servant,

A. Z.

III.

Observations on some Phenomena of Electro-Chemical Decomposition: by GEORGE JOHN SINGER, *Lecturer on Chemistry, and Natural Philosophy.*

Subject of the paper.

THE subject of the present paper occupied a considerable portion of my attention about two years since. The results of my observations have been detailed in my public lectures, but I have delayed their publication, till the progress of my inquiry should admit a systematic exposition of its objects. The contents of a paper in the November number of this Journal, by Adam Anderson, Esq., induce me to alter this determination, and to publish (out of the order I had intended) my observations on the subject of his discussion.

Mr. Anderson's remarks on the decomposition of water in separate vessels.

Mr. Anderson has supposed a difficulty in the explanation of electro-chemical decomposition, when the products are collected in *separate receivers, connected by a metallic wire.* He has also stated, that Ritter affirms, when water is decomposed in such an apparatus, the oxigen and hidrogen must pass through the *connecting wire* in opposite direction.

This not more inexplicable than oxigen and hidrogen appearing separately at a great distance in one tube.

These statements are, I believe, by no means accurate; there has not been more difficulty experienced in the explanation of the experiment described, than exists in the most simple case of electrical decomposition, viz. The impossibility of conceiving, how the same particle of water is at once acted on by wires which are remote from each other. Oxigen and hidrogen are separately produced at the extremities of a tube furnished with gold wires, even when the length of the tube exceeds three feet, and any hypothesis, calculated to explain the phenomena in this experiment, will also explain them under any other modification of the apparatus.

Ritter's opinion favourable to electric energy.

The apparatus described by Mr. Anderson as Ritter's arrangement (Journal, vol. XXX, plate 6, fig. 3,) is not described in the paper to which he refers, (Journal, 4to series, vol. IV, p. 512.) nor is the opinion ascribed to Ritter advanced in that paper, or in any other of his very numerous and interesting writings, to which I have had access. On the contrary, from the tenour of his observations, published in the Bulletin des Sciences, Journal de Physique, &c. (translations of the most important of which have appeared in this Journal;) it may be presumed his opinion favoured the hypothesis of electric energy, recently so ably supported by Dr. Davy.

Mr. Murray's account of Ritter's experiments.

Mr. Murray in the last edition of his System of Chemistry, has mentioned Ritter's experiment, accompanied by some observations, which appear to consider the permeability of the connecting wire necessary to the explanation of the experiment. But this is not advanced by Mr. Murray as the opinion of Ritter; it is couched in the terms of a conclusion;

conclusion, which he has drawn himself. "From these circumstances I am induced to believe, Mr. Anderson has taken his idea of this experiment from Mr. Murray's account, without examining the original paper; and has thus associated the arrangement and opinion of Mr. Murray with the name of Ritter. If I am wrong in this conjecture, I am indebted to Mr. Anderson for my errour; he has quoted the same paper and page of this Journal, to which Mr. Murray refers in his account; and, on a careful perusal of such paper, I find neither the experiment, nor the opinion described.

Ritter's experiments.

The experiments of Ritter, to which the above quotation refers, are detailed in a letter from a correspondent of Dr. Babington's; they are stated rather cursorily, as "some "account of the Galvanic labours in Germany." The lower part of an inverted siphon was filled with sulphuric acid, and it's legs with water. When subjected to the action of the Voltaic apparatus, oxigen appeared in one leg, hidrogen in the other. This result I have constantly obtained, and it is the same in all cases when two *separate* vessels of water are connected by any *fluid conductor* not *metallic.*

Oxigen and hidrogen appear separate in vessels connected by a fluid not metallic.

Ritter from this experiment appears to have doubted the composition of water; he is said to have arranged an apparatus, in which two portions of water, in *separate* tubes connected by gold wire, evolved respectively oxigen without hidrogen, and hidrogen without oxigen. By employing *one tube* he is also said to have procured at pleasure from *the same portion of water* either oxigen gas *alone*, or hidrogen gas *alone.* This result I have never obtained; the arrangement I employed is precisely that described in the quarto Journal as Ritter's. Two gold wires are introduced in the opposite ends of a glass tube, sulphuric acid is poured into the tube till it rises above the point of the lower wire; the upper half of the tube is filled with water; when this tube is placed in the circuit, it is said, that wire only evolves gas which is surrounded by water, and that this gas is oxigen when the wire is connected with the positive end of the battery; and hidrogen, when its contact is made with the negative. In all my trials, both wires liberated

Oxigen or hidrogen said to be obtained at pleasure from the same water.

This result never found by the author,

but a mixture

ated gas, sulphur was frequently deposited, and the gas evolved was always a mixture of oxigen and hidrogen. Yet I have no reason to suppose my experiments defective, as the acid was concentrated, and remained distinctly separate from the water.

of the two gasses, and frequently sulphur.

The apparatus represented by Mr. Anderson (plate 6, fig. 3,) is frequently employed to procure the products of electrified water separate ; and I have never observed an individual at all conversant with electro-chemical apparatus, surprised at the result. It is indeed impossible to examine the progress of an experiment of this kind, without perceiving the liberation of gas at the extremity of the wires connected with the battery, as well as at the extremities of that cemented into the receivers. *Both gasses* are *evidently* evolved from the water in *each* of the vessels A, B, C, D, but *one* only is collected; and it would be as irrational to suppose that the oxigen and hidrogen pass through the connecting wire in opposite directions, as it would to assert, that in the common experiment they pervade the voltaic battery, and the wires at its extremities.

In decomposing water in two separate vessels

both gasses evolved from the water in each, but one only collected.

Mr. Anderson however states, that, when he first repeated this experiment, he thought it necessary to adopt the above opinion, or deny the composition of water. Suspecting afterwards there might be a positive and negative point in each vessel, he arranges an apparatus, and finds his conjecture verified; but these opposite states were at the extremities of the same metallic wire, and how is this effected?

This supposed contradictory to the composition of water.

Mr. Anderson has given an explanation, which supposes the positive electricity to pass from the zinc side of the battery through the water to the remote end of the connecting wire; and the negative electricity to proceed also from the copper extremity to the remote and opposite end of this wire. According to this supposition, *the positive must pass through the negative*, or the *negative through the positive*, without restoring an *equilibrium*; though they are acknowledged to have *a strong attraction for each other*, and to be respectively of *equal intensity*. Such an idea is at variance with every thing we know of electrical action; it is not supported by any of the analogies of the science; nor is it a legitimate

The opposite electricities supposed to pass through the same wire:

but this contrary to every received theory of electricity.

Action of the Voltaic battery repeated at every interruption of the wires.

legitimate conclusion, according to the principles of *any* received theory. From the first experiments with the Voltaic battery, it has been an acknowledged fact, that the chemical changes, produced between two wires in any fluid, in one vessel, will be repeated in any number of such vessels connected together; provided the power of the battery is proportioned to the extent of the interrupted circuit. Such an arrangement was employed (soon after the invention of the battery) by Mr. Nicholson, and also by Mr. Cruickshank, who proposed the connexion of many tubes, as a means of producing a considerable quantity of gas. A similar disposition of the apparatus has been at different periods employed by most electricians; but the connexion of its phenomena with theory has been most clearly exemplied by the experiments of Mr. De Luc, in his analysis of the Voltaic pile*. This excellent philosopher has investigated with just attention the changes that occur when two tubes are employed; he has ascertained the electrical state of the different wires in the circuit, and finding the same *chemical effects* continue, when the wires underwent *changes* in their *electrical states*, he concludes, that the chemical effects do not depend on opposite electric energies, but on the passage of the electric fluid from metals to water and from water to metals; oxigen being evolved in the former case, and hidrogen in the latter.

Mr. De Luc's experiments.

A positive and negative point at each interruption of the circuit not proved.

In the present state of our knowledge it cannot therefore be said, " that the important fact of a positive and a negative point at every interruption of the circuit is established beyond all doubt." The experiment, on which this assertion is founded, is but a repetition of Cruickshank's arrangement; and proves only, that chemical effects occur at every interruption of the metallic circuit. Until these chemical effects are shown to depend on the opposite electric state of the wires, or to be inseparable from such a condition, the existence of such opposite states must be considered as purely hypothetical.

The effects produced by the circulation of a single fluid.

The experiments of Mr. De Luc appear to me a sufficient indication, that the various phenomena of electro-chemical analysis are produced by the circulation of a single electric fluid; the effects of which are modified by the nature of the bodies through which it passes. The experiments and observations I have made on this subject may be considered a sequel to this paper, and will form the subject of a future communication.

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* See Journal, vol. XXVI, pp 69, 113, 241.

Prince's Street, Cavendish Square,
Nov. 15*th*, 1811.

IV.

An Attempt to explain the Phenomena of Caloric. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

IF the following attempt to explain the phenomena attendant on caloric will not disgrace your excellent Journal, I shall feel myself much honoured by its insertion.

Elasticity of gasses both caused and destroyed by caloric.

It is a well-known fact, that caloric is the cause of the elasticity of gasses; and it is equally certain, that an electric spark, or the contact of an ignited body, will, in many cases, destroy their elasticity, and cause them to condense into a nonelastic substance. These facts may be exemplified by the decomposition and formation of water by electricity. If then, it may be asked, caloric be the cause of the elasticity of gasses; how can this elasticity be destroyed, by an addition of the same substance?

Monge's explanation of this.

This apparent anomaly has been thus explained by Monge. As gasses are rarefied by heat, the spark will cause the sudden rarefaction of that part of the mixture, through which it passes: this will cause as sudden a condensation of the adjacent parts; the atoms of oxigen and hidrogen will thus be made to approach each other; they will therefore unite, and form water.

Dr. Higgins's.

In a volume of Experiments on acetous acid, &c., published by Bryan Higgins, M. D., in 1786; I find the following, and I think more satisfactory, explanation of the phenomenon.

The particles of gasses not dissolved in caloric, but

He supposes the particles of gasses, to be surrounded with distinct atmospheres of caloric; in which the densities are reciprocally as the distances from the particles, in a dupli-

cate

surrounded by atmospheres of it.

Oxigen and hidrogen heated red hot in contact without uniting.

cate, or higher ratio. If the particles of the gasses were merely dissolved in caloric, there seems to be no reason, why they should not unite, according to their affinity for each other: for in a medium of uniform density, they would be impelled by equal, and contrary powers of the medium; there would therefore be no impediment to their motion, and consequently to their union. This being granted, the question still recurs: How does the addition of caloric facilitate their union? The answer now is obvious. By equalizing the density of the medium. It must be observed, that, if a mixture of oxigen and hidrogen be gradually heated, even to redness, in vessels which admit of their expansion; no union will take place*: for in this case, the atmospheres will preserve their relative density. But when the accumulation of caloric is sudden, as by an electric spark, the particles will not have sufficient time, to arrange it around themselves: by these means, the density of the medium, will be rendered uniform: the particles therefore, within the range of the spark, will unite; and, by their union, give out a quantity of caloric, sufficient to equalize the neighbouring atmospheres; and thus the whole of the gasses, if mixed in due proportion, will combine. Hence it appears, that the degree of ignition, which is necessary for the combustion of oxigen and hidrogen, varies directly as the density of the atmospheres; and inversely as the affinity of the particles for each other.

This theory explains all the phenomena of caloric.

This is the explanation given by Dr. Higgins; and to me it appears much more satisfactory and intelligible than the common one. But the theory, I conceive, may be extended much farther; and will even be found to be sufficient to account for all the phenomena, attendant on caloric.

No two particles of matter can touch each other.

Let us suppose, that the particles of caloric are attracted by those of all other bodies with a force, which varies inversely as the distance from the centre of these particles: and also, that they are more strongly attracted by all other bodies, than the particles of these last are by each other; or, in other words, that the particles of all bodies, have a greater affinity for caloric, than for each other. This being granted, it follows, that every particle of matter will be surrounded with an atmosphere of caloric; the density of which will increase, as the distance from the centre of the particle decreases; and this probably in a duplicate ratio. Hence, no two particles of matter can touch each other*. This is a fact, which was proved long ago by Sir I. Newton.

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* This experiment requires repetition. I do not recollect to have met with it elsewhere.

Constitution of solids.

When the attraction of the particles of a body for each other is so great, that the distance between them is less than the particles themselves, all motion among them must be prevented, and the body, consequently, will be solid. But, if the distance between the particles of a body be greater than the particles, yet, if the density of the atmospheres, which surround them, be very great, all motion may still be prevented; for it is evident, that they can only be moved by compressing the atmospheres; the density of which may be too great to admit of this compression by any force less than what would destroy the texture of the body: in this case, also, the body must be solid. It is probable, that the first case never occurs: the density of the atmospheres, therefore, is the cause of solidity. Hence the particles may be at a great distance from each other, and the specific gravity of the body vary little. The size, form, and weight of the particles, (all of which probably vary,) must also be taken into consideration.

Constitution of fluids.

Sir I. Newton supposed, that the particles of fluids were spherical; and in this manner he accounted for the facility with which they yield to the slightest impulse. But if the particles of solids be of different forms, (which may be considered as proved by Haüy,) it is difficult to conceive how they can be converted into spheres by the application of heat. But it is easy to conceive, that the accumulation of caloric will augment the atmospheres, and thus cause them to approach to the form of spheres, let the form of the particles be what it may. The same effect will

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[*] When I say, that no two particles of matter can touch each other, I speak of those which are homogeneal. It is possible, I had almost said probable, that the atoms of oxigen and hidrogen, in the state of water, do touch each other. This may perhaps help to distinguish chemical combination from cohesion, &c.

be produced, if the attraction of the particles for each other be diminished: for in this case the atmospheres will expand. Either of these causes, or both combined, will occasion the body to become fluid; for the density of the atmospheres will decrease as they extend: and thus the motion of the particles will be facilitated. The form of the particles also will influence the fluidity of the body. The more spherical these are, the less resistance will there be to their motion. Hence, to form a perfect fluid, the particles must be perfectly spherical, and placed at such a distance from each other, that the density of their atmospheres, where they come into contact, shall be 0: and this distance, probably, must be infinite. It must now be clear, that the specific gravity of fluids may be very various, and also that they may contain very variable quantities of caloric.

Constitution

If we suppose the affinity of the particles of the body to be still farther diminished; or the sphericity of their atmospheres to be increased by a fresh addition of caloric; it will necessarily assume the state of a gas. A gas then is a more perfect fluid than a liquid is. Gasses also may contain variable quantities of caloric, and vary in specific gravity.

Bodies contain different quantities of caloric.

From all that has been said, it must be evident, that a solid may contain more caloric than either a liquid or a gas; and, of course, that the quantity contained in a liquid may exceed that in a gas. But the same body, when solid, will contain less caloric than when liquid; and less when liquid than in the gaseous state. It is also clear, that the specific gravity of a solid may be less than that of a liquid; and it is even *possible*, that the specific gravity of solids and liquids, may be less than that of some gasses.

We have seen the combustion of oxigen and hidrogen explained according to this theory; let us examine, how it accords with other chemical phenomena.

Composition of nitric acid.

When azote and oxigen are mixed in due proportions, and an electric shock is passed through the mixture; the atmospheres of those particles, through which the shock passes, are rendered of a uniform density, and the particles unite. But the combustion does not spread throughout the whole mixture; because the particles which combine do not part with a sufficient quantity of caloric, to equalize the neighbouring atmospheres; the shock therefore must be repeated: Hence it follows, that nitric acid contains a large quantity of caloric: but, as its specific gravity greatly exceeds the mean of that of its component parts, the atmosphere, which surrounds its particles, must be very dense. It is needless to add, that this is confirmed by almost every experiment, in which nitric acid is employed.

Production of ammonia.

Again, azotic and hidrogen gas cannot be combined artificially, because the affinity, which these gasses have for caloric, greatly exceeds that which they have for each other. But when the hidrogen is nascent, its affinity for azote, being assisted by that of the surrounding bodies for caloric, causes their union. It may even be conceived, that the hidrogen seizes on a part of the caloric of the azote; and the whole quantity being divided between the two, the resistance is diminished; they then unite. and ammonia is produced.

Effects of compression on gasses;

The effects of compression on gasses may be thus explained. By bringing the particles nearer to each other, the density of their atmospheres is increased; they therefore exert a greater resistance, and the elasticity of the gas is increased also. If the compression be sudden and violent, a quantity of caloric is disengaged. It is exceedingly probable, that, in this case, the elasticity of the gas is injured. I have frequently thought, that I have observed this effect produced on atmospheric air; and I recollect to have seen the same remark in print, although I cannot precisely remember the place. If a mixture of oxigen and hidrogen be suddenly compressed, the heat, which is extricated, will equalize the atmospheres, and the gasses will combine. If the compression were gradually applied; it is probable, that no union would take place.

and of rarefaction.

It must be evident, that rarefaction will produce an effect contrary to that produced by compression. A part of the air being removed, the atmospheres will meet with less resistance; they will of course expand. By this means there will be occasioned, so to speak, a vacuum of caloric; it will therefore rush in from the neighbouring bodies, and thus cold will be produced; but, on readmitting the air, there will be a redundance of caloric;

the

the thermometer therefore will rise. This also agrees with experiment. I have observed it several times this morning.

Effect of increased or diminished temperature.

The expansion and contraction of gasses, by an increase or diminution of temperature, will be easily explained. As the addition of caloric will increase the density of the atmospheres, expansion must ensue. The abstraction of caloric will occasion a diminution of their density; the attraction of the particles, therefore, for each other will prevail: they will then approach; and, in many cases, condense into a liquid, or even a solid substance.

Degrees of consistency.

There are many substances, as wax, tallow, &c., which assume various degrees of consistence in passing from the solid to the fluid state. This may be accounted for by the increasing sphericity of the atmospheres, which surround the particles, of which these bodies are composed: as this increases, their motion must be facilitated.

Constitution of metals.

The malleability, tenacity, and ductility of metals, are explained in a similar manner: the atmospheres not being too dense to admit of motion, when a considerable force is applied: and the attraction between the particles being sufficiently strong to prevent their separation. When the atmospheres are too dense to permit the motion of particles by the application of a considerable force, the body must be brittle, or exceedingly hard, according as the affinity of the particles for each other varies. When the atmospheres are very far extended, the body will also be brittle, or rather friable, from the diminished attraction of the particles.

Zinc.

When zinc is cold, it is brittle, because its atmospheres are too dense to admit of the motion of its particles; a small addition of caloric renders it malleable, by increasing their sphericity: and a still greater addition renders it friable, by separating the particles too far from one another.

Hot short and cold short iron

Perhaps the difference between hot short and cold short iron may depend, in some degree, upon a similar cause.

Tempering of steel.

When ignited steel is suddenly plunged into cold water, it becomes brittle, owing to the separation of too large a quantity of caloric; but by exposure to heat, it may be made of different degrees of hardness, so as to answer various mechanical purposes. We may thus, I think, account for the effects produced by caloric in the tempering of steel: always re-

collecting,

recollecting, that motion is facilitated by the addition of caloric. Elasticity is caused by the compression of the atmospheres, and their return to their former state.

Elasticity.

Expansion of water by a diminution of temperature.

There is another phenomenon, which I believe no one has ever attempted to explain, I mean, the expansion of water by a diminution of temperature. We will suppose, with Mr. Dalton, that water is at its maximum of density at 36°. Its expansion by heat requires no explanation. Let us see how far its expansion by cold may be accounted for. It must be granted, that ice has a less affinity for caloric than water has, otherwise water would not give out caloric at the moment of its becoming solid. Hence it follows, that, when water is cooled, it must at last reach some point, at which its affinity for caloric will diminish. In most substances this probably does not happen till they reach the point of congelation: but when water is cooled below 36°, its affinity for caloric begins to diminish; the atmospheres, therefore, which surround its particles (and of course the water itself) will expand. Although the particles are nearest to each other at 36°, their attraction is not then sufficient to overcome the resistance of their atmospheres: these therefore will continue to expand, until they are so far rarefied as not to be able to resist the affinity of the particles for each other. But this will not happen till the water is cooled many degrees below the freezing point, unless by agitation, or some other means, they are brought nearer into contact.

Its expansion on solidification owing to crystallization as is the case with other bodies.

It was before stated, that the density of the atmospheres was the cause of solidity: but the specific gravity of ice is less than that of water; it also contains less caloric; hence its atmospheres must be less dense; it ought, therefore, to be more fluid than water, which is not the fact. How is this anomaly to be explained? It must be remembered, that not only ice, but all solutions of salts, &c., which form prismatic crystals, undergo the same expansion, during their transition from the fluid to the solid state. It is probably the tendency, which the particles of these bodies have to arrange themselves in prismatic forms, that causes this expansion: and the diminution of specific gravity may be occasioned by interstices between the prisms. In addition

Expansion of iron on solidification.

tion to this, it may be observed, that ice always contains a quantity of air. Dr. Priestley ascertained, that, when water, previously freed from air, was frozen in close vessels; it gave out, on thawing, a quantity of azote; and this he repeated several times, on the same water, and with the same effect*. The expansion of iron during solidification depends probably upon the detention of a quantity of caloric between the pores of the metal in an uncombined state, (i. e. not forming part of the atmospheres,) and by hammering, this may be disengaged. Those bodies, which congeal into a shapeless mass, without any appearance of crystallization, contract, as might be expected, during solidification; and this may tend to confirm the explanation given above.

Repulsion between the particles of caloric an unnecessary assumption.

I have purposely avoided making any mention of the repulsion, supposed to exist, between the particles of caloric; as I believe, that all the phenomena may be explained according to the ordinary laws of affinity. I will endeavour to explain this, in one instance; the expansion of gasses by a diminution of pressure. During the expansion, cold is produced, as was explained before; the caloric, which rushes in, arranges itself around the particles of the gas, according to its affinity for them; and thus removes them to a greater distance from each other. In addition to this, it must be recollected, that the particles will exert an attraction, beyond their own atmospheres, upon those of the neighbouring particles; this will tend to rarefy the atmospheres, and of course to expand the gasses. It is not necessary to suppose, that the particles of water and oil repel each other; if the attraction of the homogeneal particles be stronger than that of the heterogeneal, they will not unite.

No repulsion between oil and water.

Singular phenomenon.

I will now mention a phenomenon, which I once observed, and which, I think, may be explained according to this theory. During the evaporation of a solution of subcarbonate of soda, which had been exposed to a stream of sulphureous acid gas; I observed a number of globules to arise, and run about upon the surface of the solution. They did not appear to be of a gaseous, but of a liquid nature: they were exceedingly spherical, and their specific gravity so small, that they scarcely sunk at all into the liquid. The bubbles of steam, which were abundant, always receded from them; so as to leave a considerable space around them in every direction. Dr. Higgins observed a similar phenomenon, during the distillation of a mixture of sulphuric and acetous acid. I will give the description and explanation of it, in his own words.—"When the quantity of acetous acid amounted to about three ounces, and rose in the receiver so high, that the distance between its surface and the nozle of the long slender neck of the retort did not exceed a quarter of an inch, and when the drops fell at the interval of three or four seconds, each of them, rebounding after the fall, and still preserving the globular form, rolled on the acid liquor; and then, after floating in a quiescent state for five or six seconds, burst suddenly, and spread upon it. Some of these globules in their motion struck the preceding ones, and frequently the motion was communicated without either of them bursting for some seconds after they became quiescent. Afterward, when the acid in the receiver rose, and was not distant from the nozle of the retort by more than the diameter of a large drop, the liquor, which trickled down, did not discharge itself into the acid, either in the foregoing maner, or in that of a column; but it formed globules of six times the former bulk, each of which, preserving its proper form, sunk by half its diameter into the acid liquor, without mixing with it; but when the size of any globule increased so as to exceed $\frac{3}{10}$ of an inch in diameter, it then parted from the nozle, and spread on the liquor. All this looked as if each drop carried its proper atmosphere of repellent matter, which it retained for a considerable time, with a force greatly superior to the weight of a grain, for the drop could not, by reason of its mere aggregation, rebound from liquor of the same kind, and roll on it, and dip and swell in it without mixing." Dr. Higgins could not produce the same appearance, in the distillation of water, or of spirit of wine.

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* This tends to confirm Girtanner's Theory, that azote is an oxide of hidrogen.—[See also the hypothesis of Berzelius, Journal, vol. XXX, p. 270. C.]

A similar phenomenon observed by Dr. Higgins.

I will

Nitrogen expelled from the lungs in respiration.

I will add one more extract from Dr. Higgins's work, which is remarkable, as it confirms the results of the experiments of Messrs. Allen and Pepys, in the respiration of oxigen; and the conclusions which they have drawn from them. After describing the method which he employed to respire the gas, &c.; he adds, "I do not hesitate in concluding, that the former (azote) was expelled from the "lungs during the respiration, along with the matter, "which contributed to the formation of the fixable air."

I am sorry that it is not in my power to digest this paper into a more intelligible form: but it is not more than ten days since I met with Dr. Higgins's book; and it will be a considerable time before I shall have leisure to resume the subject. It has extended to a much greater length, than I at first intended: I am however conscious, that much more remains to be said, and many objections to be answered; but I am not aware of any, which may not receive a satisfactory answer, according to the principles already laid down. The importance of the subject must be my apology, for sending it in this unfinished state; and if it should be the means of drawing the attention of some able chemist to Dr. Higgins's work, and even in this remote way contribute to the improvement of the science, my intention will be fully answered.

I am, sir,
Your obliged and constant reader,

L. O. C.

Coagulation of albumen.

P. S. It may be thought, that the coagulation of albumen by heat is inconsistent with the foregoing theory. To satisfy myself of what actually does take place, during its coagulation, I poured into a glass tube a certain portion of the white of an egg, (which is albumen as pure as it can be procured,) and with a diamond marked the height to which it rose in the tube. The tube with its contents was now plunged into warm water, and the whole apparatus set upon the fire. As soon as the albumen was heated only a few degrees, I could observe bubbles of gas separating from it very abundantly; and before it had begun to coagulate, it had assumed the sparkling appearance of water impregnated with carbonic acid. After the water had boiled for some time, the albumen appeared perfectly white, and was nearly solid; (it does not become perfectly so till cold). In this state it was removed from the fire, but I was surprised to find its bulk increased, rather than diminished, for its surface was very globular. I had not however kept it out of the water more than a minute, before it had sunk considerably, so that its surface was now become concave. If in this state it had been suffered to cool, it would certainly have occupied considerably less space, than when fluid; notwithstanding the extrication of the bubbles of gas, which never rose to the surface, but always continued mixed with the albumen. When it had cooled only a few degrees, it was replaced upon the fire; its surface soon rose again, and by continuing the heat, at last burst, and a quantity of vapour made its escape. The albumen was now full of holes, occasioned either by the separation of gas, or of aqueous vapour. From the experiments of Dr. Bostock it appears, that the white of egg contains 80 per cent of water. Now when this circumstance is also taken into consideration, I think no one would urge the coagulation of albumen by heat as an objection to the foregoing theory. From the experiments which I have just related, it appears exceedingly probable, that albumen contracts during solidification, as is the case with most other bodies; and at any rate, the extrication of gas, and the quantity of water which it contains, will easily account for the apparent anomaly.

Objection from it.

It may however still be objected, If heat causes other bodies to become fluid, how can it be the cause of the coagulation of albumen? I *might* reply, that the density of the atmospheres, which surround the particles of albumen, is too great, to permit the particles themselves to approach each other sufficiently near to produce a solid; but that by the addition of caloric, these atmospheres are equalized, and thus the resistance to the motion, (and consequently to the union,) of the particles is removed, or at least diminished. I do not however urge this reason, because I believe it to be far more probable, that the albumen undergoes some chemical change, by the application of heat; and I think,

Answered.

think, that Mr. Hatchett has clearly proved, that coagulated albumen is possessed of chemical properties essentially different from those which albumen possesses in a fluid state.

Mr. Dalton's theory.

I perceive from a note in Dr. Thompson's System of Chemistry, that Mr. Dalton supposes the particles of gasses, to be surrounded with atmospheres of heat; but, as I have never seen Mr. Dalton's work, I cannot pretend to say how far he may have anticipated any thing which I have said. Dr. Higgins is undoubtedly the author of the theory, and I have endeavoured to extend it to all the chemical phenomena, which recurred to my recollection.

Mr. Gough on the elasticity of caoutchouc.

From the same excellent work, (Dr. Thompson's Chemistry) I have become acquainted with Mr. Gough's experiments on the elasticity of caoutchouc. When I wrote the paper above I was reading this work, but had not read so far as the part which gives an account of these experiments*: I was therefore very agreeably surprised, to find the same conclusion drawn from experiment, which I had previously deduced from theory, viz: that caloric is the cause of elasticity.

Effect of ignition.

Many substances (*e. g.* carbon) will not combine with oxigen, until ignited; it is perhaps needless to observe, that the addition of caloric equalizes the density of the atmospheres, which surround the particles of these bodies, and that this is the cause of their combination.

Increase of gravity of potassium when oxided accounted for.

It has been objected, by the French chemists, to the theory of Dr. Davy, respecting the metallic bases of the fixed alkalis, &c.: that, if potash (for example) were an oxide of potassium, its specific gravity, like that of all other metallic oxides, should be less than that of the metal, from which it is formed. Not to insist upon the circumstance, that potash is a hydrate of the oxide of potassium, I think it may be clearly conceived, according to the principles stated above, that oxigen may be so far condensed by the abstraction of caloric, as to increase the density of potassium, (or perhaps even of any other metal,) considerably. Accordingly we find, from the experiments of Gay-Lussac and

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* This postscript was sent some weeks after the letter to which it is annexed. C.

Thenard themselves, that potassium decomposes almost all the metallic oxides with inflammation. Perhaps this increase of specific gravity, which potassium undergoes, may be farther explained as follows. It was remarked by Messrs. Gay-Lussac and Thenard, in the experiments just alluded to, that potassium decomposes the black oxide of iron without inflammation. Let it be granted, that, during the combination of iron with oxigen, the latter only is condensed; and that the metal suffers no condensation* : and it may, I think, be easily shown, that the specific gravity of potash must exceed that of potassium; supposing the oxigen to be in the same state of condensation, as in the black oxide of iron. The specific gravity of iron is 7·8; and that of the black oxide, as nearly as I can determine it, is 4·5.—Now if the black oxide be composed, as Dr. Thompson has shown, of 78·5 iron, and 21·5 oxigen, it may be ascertained, by a very easy calculation, that the sp. gr. of the oxigen, as it exists in black oxide, is nearly 1·7.—If then we suppose, that the oxigen undergoes no farther condensation, when it combines with potassium, (which it certainly must undergo, or it would not decompose the black oxide) still it is evident, that a mere mixture of potassium, of the sp. gr. 0·6, and of oxigen in this condensed state, must be of a greater sp. gr. than the potassium alone. I have stated this, merely to show the possibility of the case, without reference to any theory whatever; but when we add the consideration, that the oxigen is still farther condensed, and that potash is combined with water, every difficulty must be entirely done away.—Permit me to suggest, that, as in alkalis the sp. gr. of the oxigen always exceeds that of the metal, and in oxides falls short of it; this may possibly be the cause of their possessing such different properties, and, in fact, constitute the difference between them.

The difference between oxides and alkalis owing to the condensation of the oxigen.

L. O. C.

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* I think no one will refuse to grant this; for it is scarcely conceivable, that the sp. gr. of iron is greater in the black oxide, than in the metallic state.

V.

On the Prevention of Damage by Lightning. In a second Letter from Mr. BENJAMIN COOK.

To Mr. NICHOLSON.

DEAR SIR,

Conductors of lightning.

Occasional failure no proof of their inutility.

Probable causes of failure,

or even mischief from them.

N a former paper, which you inserted in your very valuable Journal*, on the advantage and security that I supposed the nation would enjoy, if electric rods were placed at certain distances on the most elevated parts of the country, or if attached to the highest buildings at different places, so that the electric fluid might be carried off by the rods, as the clouds charged with the fluid passed over them; by your remark at the close of that paper, it did not seem to strike you, as promising that advantage and security it did me, and you named an instance, where the rods had failed: But if one instance, or two, have happened, where the electric rods do not appear to have had any influence on the electric fluid, so as to carry it off without injuring the buildings, this is no proof of their inutility. We ought, before we pass judgment upon them, to have known the state of the rods, and their elevation. It is very probable, that these rods had been up for many years, and nearly destroyed by rust; and perhaps in some parts the nature of the iron might have been completely changed or destroyed, and nothing left but rust; nay in some places, even the rods might have been divided, or nearly so, by the rust; so that a weak discharge of the electric fire would easily melt what was left; or they might have been carried in such directions across, or down the sides of the building, as to pass by substances possessing greater power to carry off the fluid, than such rusty decayed conductors; and thus the lightning might have been by their means conducted so as to cause the very ruin, they were intended to prevent.—Besides, the points of the conductors might have been placed very low, so that clouds overcharged with the electric fluid might have passed so near the buildings, that every part that was a conductor drew down the fire, as soon as the rods, which had lost a part of their power by rust. I say all this might have been the case, and we therefore ought not to say, that electric rods have been found ineffectual to ward off destruction.

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* Vol. XXIX, p. 305.

The subject merits investigation.

I am desirous this subject should be fairly investigated, indeed it is a national concern, and I do wish some able person would take up the subject; and if any of your correspondents could produce any one instance, where the rods, having been found in proper order and position, have failed, it would in a great measure prove their inutility. On the other hand, if any one instance could be brought forward, where they have proved beyond a doubt the protectors of a building, that without them would have suffered, some basis might be laid down to form a just idea upon.—This is certain, that we have each year to record great losses, both in property and lives, by the electric fluid; and if some plan could be devised, to remedy, if but in part, the evil experienced and complained of, great advantage and safety would be procured to society.—My opinion is, that electric rods are sure and certain preservatives to every house, where they are properly attached, if of the proper kind; and if a house can be secured, why not by the same *means* a whole parish, by a proper number of conductors?

Conductors in general defective.

But conductors are of little or no use made in the way they commonly are, of a piece of iron wire one quarter of an inch in diameter, or perhaps less; for in many I have examined they have not been so thick, some merely a strong wire. These in a year or two are nearly or quite corroded through with rust; and they are attached in a careless way, with a number of rusty points at top, directed to every point of the compass, and rising just above the chimney. It appears, that, if a rod is placed against a house or building, no matter how, the building is supposed to be safe; and if this house or building is injured by lightning, it is the rod that was to have protected it, that is declared inefficacious. These rods are generally put up by some carpenter, or builder, who knows nothing of the nature or properties of the fluid he is guarding against, and therefore brings the rod down any way that is most convenient, without considering whether it passes near or even touches any conducting substance in the building; in which case the rod, instead of protecting, is calculated to bring on the building the mischief it was intended to prevent.

How they ought to be made and applied.

Electric rods should be three quarters of an inch in diameter, according to my opinion; should not touch the building in any part by three inches; and all their fastenings to it should be by nonconductors. They should end in a single point of brass, and this point be elevated six feet at least, but ten feet if possible, above the highest chimney of the house. If the rod is not of brass, or a tube of brass, a strong brass wire ought to be wound round it, connected with the point, and passing once round the rod in the space of 12 or 18 inches, sufficient to keep the brass wire close to the iron, all down to the earth. I have no doubt upon my mind, from all the observation I have made, that electric rods of this nature will never fail to give perfect safety. Even on board vessels an iron chain, the worst of all conductors that can be called a conductor, has been known to preserve the vessel and crew. As a proof, I will quote a passage from Captain Cook's Journal of his Second Voyage Round the World. "April 25, 1774. "Otaheite—This day we had a very violent tempest. We "were obliged to get our electrical chain up to the top-"gallantmast head, to secure the masts. Removed all "the iron off the decks, and secured down all the hatches. "—As the seaman, who carried the chain up, was "coming down, he got foul of the chain, and it lightning "at the same time, he received a slight blow on the leg, "which, though it did him no harm, shook every bone "within him." Captain Cook had seen an instance of the great utility of the electrical chain in his former voyage, while at Batavia, which, being of a singular nature, I shall relate in his own words, or as they are given by Dr. Hawkesworth. "About 9 o'clock we had a dreadful storm of "thunder, lightning, and rain, October 10, 1770, du-"ring which the mainmast of one of the Dutch East "Indiamen was split, and carried away by the deck: "The main topmast and topgallantmast were shivered all

Instance of security.

Another.

" all to pieces; she had an iron spindle at the main topgallantmast head, which probably directed the stroke.—This ship lay not more than the distance of two cables length from ours, and in all probability we should have shared the same fate, but for the electrical chain, which we had but just got up, and which conducted the lightning over the side of the ship. But though we escaped the lightning, the explosion shook us like an earthquake, the chain at the same time appearing like a line of fire: a centinel was in the action of charging his piece, and the shock forced the musquet out of his hand, and broke the ramrod.—Upon this, occasion I cannot but earnestly recommend chains of the same kind to every ship, whatever be her destination; and I hope that the fate of the Dutchman will be a warning to all, who shall read this narrative, against having a spindle at the mast head."

Rods recommended for ships instead of chains.

Thus even chains have been found protectors, and if proper conductors were attached to the main topgallantmast, running all down it with a joint at the place where the mast is jointed, it would always be in its place; and I again say, I am pretty confident, that vessels would be secured from the injury they but too often sustain from lightning, as well as houses.—The rod would not be in the way of any of the rigging, and therefore I should think it would be a duty the masters of vessels owe to their sailors, as well as to the owners of the property they have on board, to be always provided against danger. I am, dear sir,

Your obedient servant,
BIRMINGHAM, B. COOK.
Caroline Street, Dec. 27, 1811.

VI.

Observations on some of the Strata in the Neighbourhood of London, and on the Fossil Remains contained in them: by JAMES PARKINSON, *Esq., Member of the Geological Society.*

(Concluded from p. 52.)

Strata interposed between the Clay and Chalk.

Strata beneath the clay.

T is almost impossible to speak with precision of the subjacent strata, which are situate between the clay and the chalk, since very considerable variations occur as to their thickness, and indeed as to the form in which their constituent parts are disposed; and since there exist but few sections, at least in the neighbourhood of the metropolis, which present a view of the strata composing this formation. They are included in the following account by Mr. Farey: "A sand stratum, of very variable thickness, next succeeds, "and lies immediately upon the chalk, in most instances, "as between Greenwich and Woolwich, on the banks of "the Thames; which has often been called the *Blackheath* "*sand*, it frequently has a bed of cherty sandstone in it, "called the gray-weathers"*.

The bottom of the clay

On the upper part of a mound at New Charlton some traces of the lowest part of the blue clay appear, covered by not more than a foot of vegetable earth. This layer of clay does not seem to exceed two feet in thickness, which, indeed, it possesses only on the top of some of those mounds, which occur so frequently as to render the surface in this district very irregular. In this clay oysters of different forms are found: some approaching to the recent species, and others longer and somewhat vaulted; but they are in general so tender, as to render it very difficult to obtain a tolerable specimen. With these also occur numerous *cerithia, turritellæ* and *cythereæ*, Lam.; all of which are in a similar state with the oysters, and appear to be shells strictly belonging to the subjacent stratum, but which, having lain uppermost, became involved in the first or lowest deposition of the blue clay.

contains shells

belonging to the stratum beneath.

Shells in great measure disintegrated.

Immediately beneath the clay there is found a line of about three or four inches of the preceeding shells imbedded in a mass of calcareous matter, the result of their disintegration. Beneath this are numerous alternating layers of shells, marl, and pebbles, for about twelve or fifteen feet. The shells are those which have been already mentioned; but are very rarely to be met with whole, and when entire are so brittle as to be extricated with much difficulty. In some of these layers scarcely any thing but the mere fragments of shells are to be found, and in others a calcareous powder only is left.

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* Report on Derbyshire, &c. vol. I, p. 111.

The pebbles are almost all of a roundish oval form, many of them being striped, but differing from those of the superior stratum, in being seldom broken, in there being few large ramose masses, and in their not bearing any marks or traces of organization. Many of these pebbles are passing into a state of decomposition, whence they have in some degree the appearance of having been subjected to the action of fire: small fragments of shells are every where dispersed amongst them.

Pebbles.

Beneath the pebbles is a stratum of light fawn coloured sand of about ten feet in depth, and immediately under this is the stratum of white sand, which is about five and thirty feet deep, and is here seen resting immediately on the chalk.

Sand beneath these.

At Plumstead, about a mile distant in a south-eastern direction, there is a pit in which the shells, about two years ago, were to be obtained in a much better state of preservation than at New Charlton; but this seam of shells, as the pit has been dug farther in, has by degrees become so narrow, as to be now nearly lost. In this pit, not only the shells already mentioned were found, but many tolerably perfect specimens of *calyptræa trochiformis*, Lam., *trochus apertus*, Brander., *arcæ glycemeres*, *arcæ Naticæ*, and many minute shells in good preservation. All these shells appear to have entirely lost their animal matter, and, not having become imbued with any connecting impregnation, they are extremely brittle. On examination with a lens it also appears, that in most of the specimens nothing of their original surface remains, it having been every where indented with impressions of the surrounding minute sand, made while the shells were in a softened state. This circumstance is particularly evinced in the *cyclades*, in which a particular character in the hinge was thus concealed; in a mass of these shells from the Isle of Wight, it appears, that the lateral teeth are crenulated, somewhat similar to those of the *mactra solida* in the gravel stratum; but in the *cyclades* of Plumstead, this was not discoverable from the injuries, which their surface had sustained from the sand.

Shells at Plumstead.

Particular character in the hinge of one concealed.

The fossils of this stratum evidently agree with those found by Lamarck and Mr. De France, above the chalk at Grignon, Courtagnon,

The same fossils found in France, and in the Isle of Wight.

Courtagnon, &c.; and they have been just shown, incidentally, to exist in the Isle of Wight. In an eastern and southern direction from London this stratum with its fossils is frequently discovered.

Shells about Crayford.

On the heath near Crayford, about four miles eastward of Charlton, long vaulted oysters are found similar to those already mentioned. About two miles farther, in the parish of Stone, is *Cockle-shell-bank*, so called, as Mr. Thorpe, the author of *Custumale Roffense*, says, p. 254 of that work, "from "the great number of small shells there observable." These are the *cyclades* already spoken of, and which Mr. John Latham, author of *The general Synopsis of Birds*, thought bore some resemblance to *tellina cornea*, Linn., *Histor. Conchyl.* of Lister, tab. 159, fig. 14. Mr. Latham here also met with a species of *cerithium*, and another of *turritella.* Fragments of these shells are also frequently turned up with the plough in that neighbourhood. They have likewise been found at Dartford, at Bexley, and at Bromley, to the southward.

Large mass of stone filled with shells.

Mr. Thorpe also relates, that, in the parish of Stone, there was a large mass of stone, of some hundreds weight, full of shells, which was brought from a field, and used as a bridge or stepway over a drain in the farm-yard. (*Custumale Roffense*, p. 255.)

Course shelly limestone.

In several spots in the neighbourhood of Bromley, stone is found near the surface, formed of oyster-shells, still adhering to the pebbles to which they were attached, and which are similar to those which have been just described, as occurring at Plumstead and at Charlton: the whole being formed by a calcareous cement into a coarse shelly limestone containing numerous pebbles. The only quarry of this stone, which has been yet worked, is in the grounds of Claude Scott, Esq. The opening hitherto made is but small; it is however sufficient to show, that the stratum here worked has suffered some degree of displacement, as it dips with an angle of about forty-five degrees.

Stratum of sand over the chalk,

At Feversham, over the chalk, Mr. Francis Crow has discovered a bed of dark brown sand, slightly agglutinated by a siliceous cement, and intermixed with a small portion of clay. In this stratum, which has been hitherto but little explored, he has found, in a siliceous state, specimens of *strombus pes pelicani*, and a species of *cucullæa*, nearly resembling those which are met with in the Black-down whetstone pits.

and frequently patches of potter's clay, of different kinds.

Patches of plastic clay are frequently found over the chalk: some of these are yellow, and employed for the common sorts of pottery; but others are white, or grayish white, and are used for finer purposes. The coarser clay is very frequently met with, nor are the finer kinds of very rare occurrence. In the Isle of Wight two species of plastic white clay are worked for the purpose of making tobacco-pipes. A similar clay, which is used for making gallipots, is dug from the banks of the Medway. A fine light ash-coloured, nearly white clay, which is employed in pottery-works, is also dug at Cheam near Epsom in Surry.

Upper or flinty chalk, a very thick stratum.

The *upper or flinty chalk*, which is the next older stratnm, is extremely thick, forming stupendous cliffs upwards of six hundred and fifty feet high, on the south-eastern coasts of the island. It extends nearly through almost all that part of the island, which lies south of a line supposed to be drawn from Dorchester in the County of Dorset to Flamborough-head in Yorkshire.

The flint in parallel layers.

In this stratum there is a great quantity of flint, chiefly in irregularly formed nodules, disposed in layers, which preserve a parallelism with each other and with continuous seams of flint, sometimes not exceeding half an inch in thickness. The chalk contains a fine sand, which may be separated by washing*.

Fossils of this stratum chiefly peculiar to it. Closely allied to those in France.

The fossils of this stratum are for the most part peculiar to it; very few of them being found in any other. They also appear to agree very closely with those species found in the chalk of France, by Messrs. De France, Cuvier, and Brongniart. The number of fossils noticed by these gentlemen amounts to fifty; but they have yet only particularised a part of them. These are here compared with what appear to be the correspondent fossils in the English part of this stratum; and some others are also pointed out, which these gentlemen have not yet mentioned as being found in the neighbourhood of Paris.

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* The chalk in the neighbourhood of Paris contains, according to Mr. Bouillon La Grange, magnesia 0·11, and Silex 0·19.

In the French stratum there occur,

Fossils in the French stratum.

Two *lituolites.* No species of this genus is noticed as having been seen in our English chalk. But research has not been made with the necessary precision.

Three *vermiculites.* The fossil figured Org. Rem. vol. III, pl. VII, fig. 11, was considered as a vermiculite, until by removal of the chalk, and opening different specimens, it was found to be a chambered and an adherent shell. Should these gentlemen not have perceived these circumstances in the specimens they met with, they would certainly regard this fossil as a vermiculite. It must also be observed, that, from the different forms in which the spiral part is disposed, its division into two or three species might be authorised.

Belemnites. These, according to Mr. De France, are different from those which accompany the *ammonites* of the compact limestone. The *belemnites* of our chalk are smaller than those of the limestone, beside which they are different in form, being narrower and more elongated. But Mr. De France may also have confounded with them the spines of the *echinus,* which so closely resembles the *belemnite:* if that gentlemen should not have met with perfect specimens, he might not be able to remark the difference between these two fossils. The characters, which he has noticed, are however sufficient to lead to the belief of a correspondence between the French and English fossils.

Fragments of a thick shell of a fibrous structure. The doubts expressed respecting the nature of this shell, and the observations made with regard to it, offer another strong point of agreement between the shells of the two strata. The shell here alluded to is most probably that represented Org. Rem. III, pl. V, fig. 3; the structure of which agrees exactly with that mentioned as found in the French stratum of chalk. That shell is however described as being of a tubular form; it is therefore right to observe, that fossil *pinnæ* do sometimes possess this peculiar structure.

A *muscle.* No instance appears in which any shell of this genus has been found in our chalk.

Two *oysters.* The Kentish chalk-pits yield at least three species of this genus. One of them bearing very much the form and appearance of *ostrea edulis,* but being only about a fourth a fourth of its size; one smaller, the serrated edge of which places it in the family of *cristæ galli*; and the third still smaller, not half an inch in length, crenulated on each side of the hinge.

A species of *pecten*. There are two or three small species of *pecten* in the English chalk; beside a shell with long slender spines, which may be safely classed with the pecten.

A *crania (anomia craniolaris*, Linn., *crania personata*, Lam.). This fossil is not known in the English chalk; nor indeed could it be easily ascertained, unless the inferior valve happened to be well displayed.

Three *terebratulæ*. *T. sulcata* and a terebratula agreeing with *anomia terebratula* Linn. are frequently found in our chalk; and sometimes another species, hardly half an inch in length, with remarkably acute and well defined ribs.

A *spirorbis*. Traces of these shells are frequently found on the surface of the *echinitæ*.

Ananchitæ, *(echinus ovatus)*, the crustaceous covering of which, it is remarked by Messrs. Cuvier and Brongniart, remains calcareous, and has assumed a sparry texture, while the middle alone is changed into silex. No actual change has however taken place. as far as respects the flinty part of the fossil, the flint having merely filled up the hollow of the sparry crustaceous covering, This fossil is frequently found in the English chalk.

Porpitæ. These also occur in the English chalk.

Five or six different fossil bodies called by the French oryctologists *polypiers*, one appearing to belong to the genus *caryophyllæa*. Several of these bodies, from the English chalk, have been figured in the Org. Rem. vol. II, Pl. XIII, fig 70 to 79.

Another is supposed to belong to the genus *millepora*. This is generally brown, and is in the state of oxidized iron, as resulting from the decomposition of pyrites. These fossils exist in the Wiltshire soft chalk.

Lastly, *shark's teeth*. These also occur frequently in the English stratum.

Many more fossils in the French stratum, and in the English.

Messrs. Cuvier and Brongniart state, that there are many more fossils in the chalk stratum of France, than those which have been just referred to. This is also the case with the fossils of the English chalk; since the following may be enumerated as occurring in this stratum. *Rugous palates*, and though rarely, the *scales* and *vertebræ of fishes*. Three or four species of *stellæ marinæ*. A *long saccular bivalve*, with an uncommonly thin shell, of which so little has been hitherto saved, as not to give a chance of gaining a knowledge of its general form or the structure of its hinge. *A bivalve*, which approaches to a circular form, but is so thin as to afford but little hope of discovering its genus. *A bivalve, nearly circular, the margin turning upwards so as to give it a patella or disk form, with numerous long processes passing from the margin and external surface, and fixing it to other bodies. A small pecten with sharp angulated ribs*, not exceeding a quarter of an inch in length. *A bivalve*, not an eighth of an inch in length, finely striated longitudinally, bearing a bright polish, and seemingly possessing its original light brown colour. *Plates of the tortoise echinite*, and several remains apparently of other species of this genus.

When to these are added the remains of various *echini*, such as *conulites*, *cassidites*, and *spatangites*, and the different *spines* of *echini* which are found in this stratum; and when it is also considered, that the present account is drawn up almost entirely from the productions of chalk cliffs of not more than two miles in length, it will not be difficult to conceive, that the number of these fossils is not less in the English than in the French chalk.

These fossils imbedded by a gradual deposition.

The state, in which these fossils are found, plainly evinces; that the matrix in which they are imbedded was formed by a gradual deposition, which entombed these animals while living in their native beds. The fine and delicate spinous projections of the shells are unbroken, and the spines are still found adhering to the crustaceous coverings of the *echini*; neither of which circumstances could have occurred had these bodies been suddenly and rudely overwhelmed by these investing depositions, or had they been brought hither from distant spots.

Objection answered.

It may be said, that the specimens possessing the characters here alluded to are rare. With respect to the spinous shells, however, they certainly occur often, although it is almost impossible to extricate them unbroken from their surrounding chalk; and the rarity of the specimens of *echinites* with their attached spines depends in a great measure on the mode, in which these specimens are obtained. The specimens seen in cabinets are seldom found by the naturalist himself, but are preserved by the work people, who break the chalk, when any uncommon appearances catch their eye. But it frequently happens, that these marks are not seen until the piece is broken by their tool, and with it perhaps, the entire animal.

Farther proof.

The perfect state of the surfaces of the chalk fossils proves also, that this deposition proceeded from the surrounding fluid; and that it was not derived from the immediate action of any chemical agent on the shells and other calcareous coverings of the animals living at the bottom of the sea. In the fossil animal bodies found in chalk, not the least diminution of the sharpness of their ridges or points is observable, nor is the least dulness of the delicate lines and embossments of the crusts, or of the spines of the *echini*, to be detected.

Chalk and flint deposited alternately.

That the deposition af chalk and of flint was sometimes alternate, and even, as it is expressed by Messrs. Cuvier and Brongniart, *periodical*, appears from the seams or strata of flinty nodules, and particularly from the widely extended flat or tabular flinty despositions interposed between the chalk.

But the chalk permeated by the silex some time after its deposition.

But that the chalk was permeated by the silex at some distance of time after the deposition of the former, seems also to be proved by the state of the fossils of this stratum. There does not appear to be a single instance, in which the animal remains are impregnated with silex. On the contrary, the substance of all these fossils has become calcareous spar, and their cavities have been filled with flint; thus plainly evincing, that sufficient time must have elapsed for the crystallization of the calcareous spar, previously to the infiltration of the flint.

The flint has never mixed with the chalk, but the chalk has with the flint.

It may not be improper to remark, that in no instance does the flint, although in contact with the calcareous spar, appear to have become mixed with it. The reverse of this is the case with the chalk, since this latter may be seen in almost every degree of union with the flint; from being blended with its substance, to being merely united with its surface, and forming the white coat of the flint. It has been, without doubt, from certain appearences resulting from this union, that Mr. Carrosi and others have been led to believe in the change of lime to flint.

Theory of the formation of imbedded flint.

There can be hardly any hesitation in agreeing with Mr. Jameson, that the most probable explanation of the formation of imbedded flint is that which was first proposed by Werner; " that, during the deposition of chalk, air was " evolved, which, in endeavouring to escape, formed irre- " gular cavities, that were afterward filled up, by infiltra- " tion, with flint"*. The decomposition of the softer parts of the animals, which were thus entombed, may be considered as a very probable source of a part of those gaseous matters, which formed these cavities: and the connection of the animal remains with these nodules of flint is easily explained, by supposing the shells, crusts of the *echini*, &c., to have projected into these cavities, or to have been adherent to their sides, at the period at which this infiltration took place.

The flint formed by crystallization

That the separation and deposition of the matter forming these siliceous nodules have been the work of crystallization, is rendered evident by the cavities left either in these nodules, or in the fossils, being generally lined with quartz crystals.

A difficulty answered.

While endeavouring thus to explain the formation of these flinty nodules, and the filling up of the cavities of the fossils with flint, a difficulty arises from observing these bodies insulated as it were in their bed of chalk: it not being easy to conceive, how so copious an infiltration should have taken place into these cavities, while the surrounding chalk should only have received a slight intermixture of siliceous grains.

Formation of calcareous stalactite.

Something analogous is however observable in the formation of the calcareous stalactite; since in those caverns, in which these concretions have been forming for a very long period, the infiltration, by which they are formed, is found to continue to the present day; proving, that the interstices of the superincumbent stone have not yet been filled by the concreting of the earthy particles held in solution in the percolating fluid, by the crystallization of which these bodies have been formed, and are now augmenting.

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* System of Mineralogy by Prof. Jameson, vol. I, p. 172.

Oberstein nodules of agate.

The Oberstein nodules of agate appear to have been formed under somewhat similar circumstances; since it is in general evident from their external surfaces, that they also have had very little adherence to their matrices, which would hardly have been the case, had these been highly impregnated with silex.

Hard chalk without flint.

The hard chalk lies immediately beneath the soft chalk. In this stratum there are no flint nodules. "Its beds," according to Mr. Farey, "increase in hardness, until near "the bottom, where a whitish freestone is dug, at Totternhoe in Bedfordshire, and at numerous other places; that "brought from Ryegate and other quarries of this stratum, "south of London, is used as a fire-stone*."

A different formation.

It has been generally supposed, that these two strata of chalk are of one formation: but not only the absence of the flints, but the characters of their fossils, prove them to be of distinct formations. No fossils indeed are marked by more decidedly peculiar characters than those of this stratum; since hardly a single fossil has been found in it, which has been met with in the soft chalk, or any other stratum.

Its fossils peculiar.

Ammonites first occur in it.

It is in this chalk, that the genus *ammonites* is first met with; or, in other words, it appears, that the water, which formed this stratum, was that in which this genus last existed, no traces of it having been seen in the soft chalk, or in the other superior strata. The chief, and perhaps the only circular species of this genus, which has been found in this stratum, is of a large size, with nodular projections on its sides, toward the back, which is generally flat. This fossil appears to be of a different species from any of those, that are found in the subjacent strata.

One species found in no other.

One with almost a generic difference.

It is very remarkable, that in this stratum, the last in which the genus *ammonites* is met with, so remarkable a deviation from the original form of the genus should occur, as almost to claim its being considered as the characteristic of another genus. In the fossil here referred to, which possesses all the other characters of *ammonites*, the spiral coil is disposed in a form rather approaching to that of the oval than the circle*.

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* Report on Derbyshire, &c., p. 112.

A still more remarkable deviation.

In another fossil of this stratum a still more extraordinary deviation exists. This fossil possesses the concamerations and the foliaceous sutures of the cornu ammonis; but, instead of being spirally coiled, it has its ends turned toward each other, somewhat in the form of a canoe. This peculiar form has led to the placing of this fossil under a separate genus, which has been named *scaphites*†.

Extent of this stratum.

Of the extent of this stratum no correct account has been given; but there is sufficient reason for believing, that it accompanies the other chalk in its range through this island. It also appears, that its peculiar fossils exist in it at very considerable distances. Thus the *oval ammonite*, which is found in the Sussex hills, likewise occurs in the hard chalk of Wiltshire; and the *scaphites*, another inhabitant of the Sussex hills, has also been discovered in Dorsetshire.

The strata above the chalk in England differ from those in France.

On comparing the preceding sketch with the Essay on the Mineralogical Geography of the neighbourhood of Paris, by Messrs. Cuvier and Brongniart, some important variations will be perceived between the strata found above the chalk in this island and in France. In France, the strata above the chalk differ both in number and quality from those, which have been hitherto observed in a similar situation in England. In France too, several strata of sand and sandstones exist above the strata of the gravel formation, which in this island appear to be highest.

Attempt to account for this.

The first of these differences appear to result chiefly from the existence of numerous beds or patches, the formation of which must have depended on certain local circumstances, such as the existence of fresh or salt water lakes, at the period of the drying up of a former ocean; the different chemical combinations, which might thence have taken place; &c. But the occurrence of such variations can hardly be considered as interrupting the continuity of the stratification.

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* Organic Remains, vol. III, pl. IX, fig. 6.

† Organic Remains, vol. III, pl. X, fig. 10 and [illegible].

Indeed when it is considered, that in France much more frequent opportunities are afforded of examining the stratification immediately above the chalk than in England, it will not be regarded as improbable, that several of these beds or patches may exist here, the discovery of which would render the accordance of the two series of strata much more close.

Even from the examinations, which have been already made, the identity of the French and English chalk is established. The British strata above the chalk are also found to contain patches of plastic clay, of most of the varieties mentioned in the French strata, as well as patches of coarse limestone, with its accompanying sand and its peculiar fossil shells, such as are found to exist in the corresponding French strata.

The other difference, the existence, in France, of beds of sand and of sandstone above those of gravel, which are the highest strata of this island, is very remarkable. May it not be attributable to the abruption, from this island, of the superior strata or beds of this formation, by that catastrophe, instances of the astonishing force of which have been already noticed?

VII.

Experiments on Muriatic Acid Gas: by J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR, *Edinburgh, Dec.* 28, 1811.

Reason of delay.

THE state of my health has not allowed me to send you an earlier account of the farther experiments on the nature of muriatic and oximuriatic acids, which I announced in my last communication. I now beg leave to submit them to the attention of your readers.

Two modes of deciding the nature of oximuriatic acid.

I have already observed (Journal, vol. XXVIII, p. 139,) that there are two modes of investigation, by which the question at present under discussion with regard to the nature

Fig. 2.

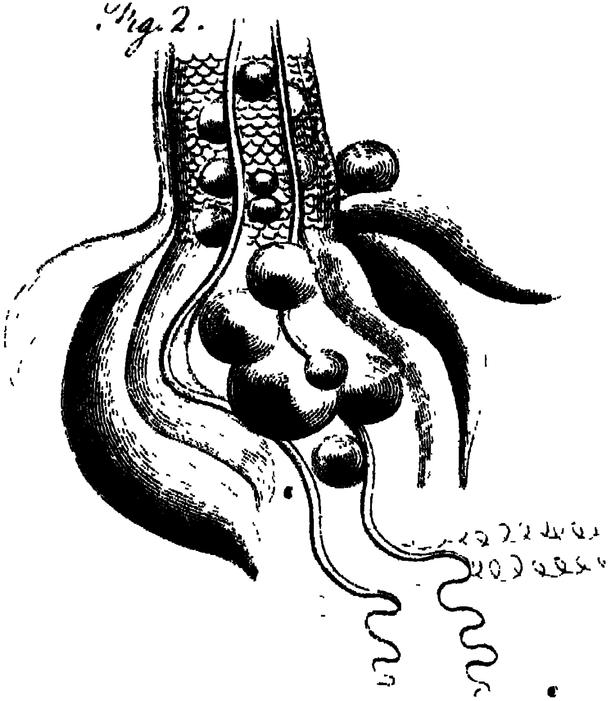
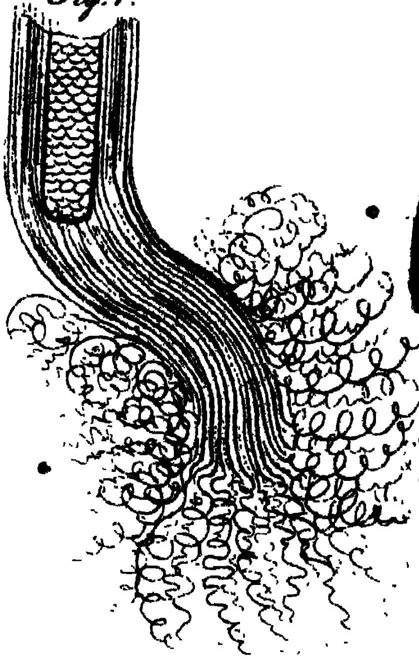


Fig. 1.



Fig

Fig. 3.

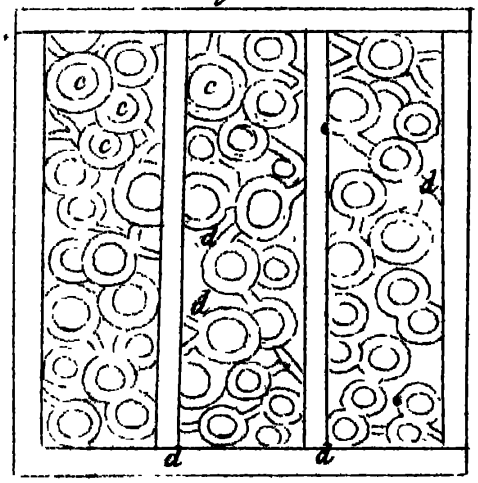
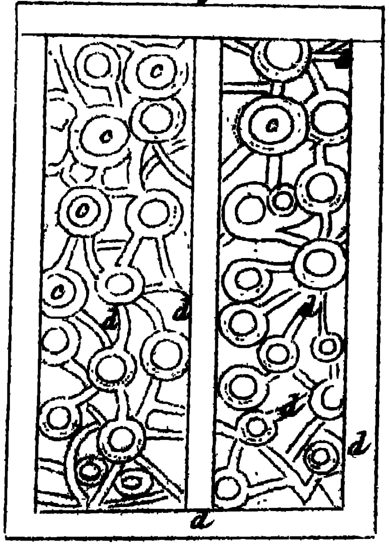
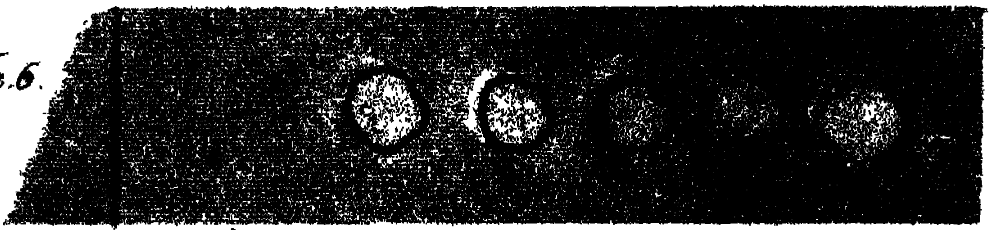


Fig. 4.



Sugar in Serum of Blood.

Fig. 6.



A

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AND

THE ARTS.

MARCH, 1812.

ARTICLE I.

On the different Sorts of Wood, with some Remarks on the Work of Du Thouars. *In a Letter from Mrs.* Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

On the treatise of Aubert du Thouars.

I Know not whether the account I have given of the mechanism of Botany has convinced my readers, or done justice to my subject; the latter is indeed impossible. I shall however (though thoroughly sensible of my deficiencies) renew the discussion as soon as the spring bestows a fresh return of her treasures for dissection. In the mean time I shall give a farther idea respecting the formation of *wood*, in some measure answering to the new Treatise on Vegetation, received from France, and written by Aubert du Thouars, celebrated for his studies in physiology, and who appears (like myself) to have taken nature for his guide, and left books for a future consideration; thinking it of greater consequence to ascertain a few absolute facts, than to collect a confused mass of uncertain details, that lead to no end.

The bud another kind of seed.

But the root and wood differ in many respects.

No returning vessels.

Each leaf has not a separate wood vessel.

Du Thouars agrees with me, that each bud is only another kind of seed, having its cotyledons the same, and wood vessels reaching from the bud to the root, and ending in a radicle, so that each tree may be esteemed (in this respect) a collection of plants, really distinct from each other, though joined under a double cover. But in my opinion he carries this idea much too far in giving the same appellation to both root and wood, for they certainly greatly differ in many respects, which he seems not to have noticed, beside the former having a double vessel, till it joins the radicle, while the wood vessel is single; as I have before described in my former account of this substance. He is also of my opinion respecting the supposed circulation of sap, which he appears persuaded does not exist, the liquor flowing to the terminating branches only, to give life to fresh matter, shooting from every extremity where the flow of sap will allow it to form; indeed, we are now so well acquainted with the different parts of the stem, that, if there were any returning vessels, they could not (I think) but be known. But he is undoubtedly mistaken in saying, that each leaf has a wood vessel, as well as the bud; for, if this was true, the stem must be as wide again. I have traced this part with such exact attention, as not easily to be mistaken; and have always found, that the set of wood vessels, after meandering all over the leaf, returns on the upper side of the stalk, and thus enters another leaf, that the same collection may serve many hundreds. It is not difficult to follow them, and is most convincing.

Where the flower bud is formed.

There is nothing that gives me more astonishment than the blindness of physiologists respecting the formation of the flower bud. Du Thouars, as well as Mr. K., thinks it is formed in the new wood, next the bark, and believes the bud is generated by the sudden mixture of the wood and bark, as if they did not run side by side throughout the plant; a strange mistake! I confess it is this discovery "of the shooting of the bud" I am most proud of, first because it is the foundation of many important facts, that lead to consequences of no little moment; secondly, because it is so very plain, so easily seen, that, strip off the bark, and a child would acknowledge and understand it; as the buds appear coming out from the interior of the wood in every part. But how many truths does this substantiate! It proves which is the vital part of a plant; that the impregnating line, which runs into the seed, is likewise found running next the pith in the stem; that this is the line of life, from which also all buds proceed; that the seed and bud are the same thing, at least differing in trifles only; and that they both owe their existence to this same vital part, one shooting in the flower, the other in the stem of a plant. In herbaceous plants this same line runs within the pith, but equally gives life to bud and seed; hence the truth of Linnæus's observation, when he gave such consequence to the pith of some plants.

Consequences deduced from this.

Manner in which the bud is first united with the sap-vessel that nourishes it.

There is something so curious in the manner in which the bud is first united with the sap vessel that nourishes it, that, as I have not exactly shown the process, I shall give it here. I have described the manner in which the line of life first generates the bud by forming a knot on that line, and breaking the outward ends. Each end then becomes a bud, and it is the business of the wood to form a covered way for the passage of the bud to the exterior, which it does by bending some and raising other parts of its vessels, in the middle of which the bud passes to its cradle; but no sooner does the knot form on the line, than it becomes a signal for the root to send up a vessel for the nourishment of the new bud, and by the time it reaches its cradle, this vessel (loaded with sap) arrives at the same place, and fastens itself to the bud, affording it that nourishment the milky juice of the albumen could no longer give it. It is exactly the same process as that in the seed, where the milky albumen first supports, and is then succeeded by the nourishing vessels.

The wood the only part that conveys the sap for nourishing the plant.

I shall now give a curious proof, that the wood is the only part which carries the sap for the nourishment of the plant; and that the plant dies, if the wood vessel is lost. In tracing the various diseases of plants, especially in our kitchen vegetables, it occurred to me to examine thoroughly that which is called smut in potatoes; and compare it with

Diseases of plants.

the disorders which are found in the brocoli and turnip. In the brocoli the first apparent symptoms are a shrivelling and drooping in the stem of the plant; it for some time languishes like a consumptive patient, and then dies after a long illness. Curious to find what could be the cause of this disorder, and its effects on the interior of the plant, and why the plant ceased to gain that nourishment, which the earth seemed so fitted to give; I resolved to dissect the plant with care. To prepare myself for thoroughly understanding it, I took a brocoli plant growing in other ground, and perfectly healthy, dissecting and drawing it, as I give it at fig. 1, plate V. It will be seen how many radicles it has, how many wood vessels, both in the root and stem. I now laid open the diseased plant with the greatest care: but how excessive was my surprise to find, that almost all the wood vessels had disappeared, though the plant was twice the size of the healthy brocoli, and of a bulbous form in the root, see fig. 2; that in place of the wood vessels, I could see only tubercles filled with water, and that, instead of near a hundred radicles, two solitary ones, with their appropriate wood vessels, were all the plant had to bring it support. The bulbs of water were composed of a loose matter something like the pith of trees, but very large, and without one ligneous particle; in short the complaint appears like a dropsy in the plant. From various trials it was plain to be seen, that the defect arose from the earth; since if pease, beans, vetches, &c. were substituted for the brocoli, turnip, or potato, they would grow admirably, but the same disorder would equally attach to those vegetables just named, if placed in that earth. The cause then was the failure of suitable nourishment for those plants in that ground; and its consequences were the decay of the parts intended to bring that sort of sap, which the ground was not able to bestow. This shows also, that there is a great variety in the sap, though we are not able to discover it; indeed in our trials so much of the most important and delicate essence may evaporate, during the first attempt to ascertain the ingredients, that we cannot thoroughly trust to our knowing all that enters into the composition of this astonishing mixture. I cannot conceive that any thing but decay

Disorder of the brocoli.

The cause in the earth and not in the plant.

Ingredients of the sap not known.

decay could cause the disappearance of the wood vessels; and am more confirmed in this idea, as I have always found that it is the constant consequence of the stoppage of the sap, whenever it takes place during the life of the plant. This I before showed, when I endeavoured to prove how mistaken physiologists were in supposing that part of the stem of the tree was void of sap, and was only filled in the newest part.

The ground too cold and moist.

On summing up the evidence I should conclude, that the ground is too cold and moist for these vegetables. On dissecting turnips and potatoes growing in the same ground, I find it exactly the same disorder as that in the brocoli, that is a total disappearance of the ligneous parts, both in stem and root. The tubercles of water mentioned in the brocoli, turnip, &c. are always full of stinking water, as the putridity is just beginning. May not the disorder of these plants in some measure serve to prove, with the rest of the facts already adduced in my former letters, that the sap is conveyed by the wood alone for the nourishment of the plant? I think the body of evidence I have at different times given now nearly amounts to certainty, that the sap runs in the wood alone, and does not circulate. This disorder must not be mistaken for that called the scab in potatoes, which is a malady that proceeds from a cryptogamian plant, first growing on the outside of the potato, in which insects fix and lay their eggs, to insure food to their hatching young; which soon dive into the interior, and cover the root with blotches.

Sap conveyed by the wood alone.

Scab in potatoes.

The next part which belongs to the wood, and which I wish thoroughly to explain, is that which appertains to the balls. I have shown, that there are in plants two sorts, one which generally regulates the mechanism of plants, (the account of which I gave in my last letter on mechanical botany): the other the ball found in the wood of trees. When a bud, formed of the knot of the line of life, and passing through its covered way to the exterior, is by any accident severed from that line, the bud stops, and can proceed no farther, though the wood vessel joins to it. In time the albumen, which surrounds it, changes to wood, and the bud, though its interior never grows, yet continues

Ball found in the wood of trees.

to increase in wood, as long as the hardened parts about it will permit, adding one little row of wood each year, but this soon ceases. It is perfectly detached from the rest of the plant; and afterward pushed towards the exterior by the growing part of the plant, while the sap continues to circulate round it and within it. The constant pressure these balls receive makes them grow to an inconceivable hardness; and, when taken out, I have found them from twelve inches to a quarter of an inch in diameter, and so regularly formed, (see fig. 5) that had I not taken them myself from the tree, I should have been persuaded they were just turned in a lathe. Some are round, but with a single wood vessel attached to them; some formed like a spinning top. Here is an old tree, that has formerly been in a hedge-row, that has three balls about twelve inches in diameter. Carpenters, when they find them, use them as beads, cogs of wheels, or for any purpose that requires extreme hardness.

Balls in the wood.

The forming of hungry wood.

The next peculiarity of the wood I have never yet thoroughly explained is the exact reverse, in effect, of the ball; and the cause of both can be described by a drawing: it is what the French call hungry wood, it proceeds from some accident, a severe season, lightning, or injury the tree has received. Some wood is much more liable to it than others. It is a formation that is quickly finished, but that stops many of the sap vessels, so that the wood is soft and poor. I have often found a piece in the middle of beautiful perfect wood so diseased. Hungry branches are often seen shooting from the roots of trees. The rose, the viburnum, the barberry, and many shrubs as well as trees, are subject to this defect. Among trees the plane, the ash, and the lime in particular are most liable to it; it is to be known by its difference of appearance. To show it I shall draw a piece of solid good oak, and a piece so affected, having these specimens now in my possession. Fig. 3 is the healthy piece, fig. 4 the diseased oak: *c c* are the sap vessels, *d d* are the intermediate parts of both. When young, the wood is wider between the sap vessels, but not near so far apart as the diseased wood, which never appears to contract, as all perfect wood does. It is this effect, that has made many physiologists think, that the sap vessels dried up, when the wood

No wood without sap.

wood became old; but it only leaves the intermediate parts, which constantly contract. The sap vessels are rather enlarged than lessened by age; and grow vastly firmer by the strong support they thus gain by reduction.

Trifling difference in the albumen.

I shall now turn to the wood of annual or herbaceous plants: it is formed of two sorts, those which like the shrub have only a narrow piece of ligneous matter; and those the wood of which is formed in compartments, something like the manner of the leaf stalks, that is, round vessels of wood surrounded by albumen, which altogether appear infinitely larger than the common vessels so denominated, the spiral wire being enclosed within the wood. These vessels are set like spots in the circular pith; the number of sap vessels increases, if the plants last a long season, as well as the rows in the herbaceous vegetables, if fine weather prolong their existence: this I have before hinted at, in a former letter, Vol. XXVIII, p. 249. It would seem that this albumen, as well as that which surrounds the ligneous vessels in the leaf, never became wood; for let the vegetables be ever so old, it still retains the clear and unformed appearance it has at the first moment; and let it be where it will, it is always to be known. It is this we call nourishing vessels in the leaf. I think it should be called by the name of clear albumen, to distinguish it from that which afterward becomes wood. I have examined both; and, as far as the eye can judge, they appear perfectly the same; but in taste the clear albumen, which is that found in the seed, the leaf stalk, and herbaceous plants, is bitter, which the other is not, the other albumen is the new row of wood in trees and the foundation of the seed, with the first formation of the embryo. In all these the sap vessels afterward shoot. When I tried both by decomposition the residuum was the same; indeed the matter I can get for trial is so little, it is hardly a fair chemical process,

I should have continued to give an account of the ideas of Aubert du Thouars, could I have thoroughly understood the rest of his opinions; but he is not so intelligible as de Mirbel; and (if I may be allowed to say so) has rather too much given way to system and imagination. I should suppose it is necessary to pass from step to step in this study, perfectly distinguishing first the separate parts of a plant, and then the different sorts of vegetables, and keep to this; for if the seed is not exactly divided from the leaf, the stem from the root, and so on to the rest; and if the tree is not distinguished from the shrub; and the semiplant, and herbaceous plant, from the cryptogamia; it must inevitably create confusion, as they differ so essentially in form and manner of growing. It is also most desirable to give but one part at a time explaining this with a print, that may make it understood, or the most perspicuous writer, on so dark a subject, will want elucidation. But this gentleman, (though he began so well) has launched into some system, which he seems to hold out, I think, as some mathematical demonstration in the vegetable world. I regret that he has not divided it from his physiological labours, as both might have profited by this arrangement, I have however so much to apologize for myself, that I am the last person, that should criticise others.

Rest of du Thouars work.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

II.

On the Action of Elastic Fluids on dead Animal Flesh: by Mr. HILDEBRANDT.*

The putrescency of bodies must be much influenced by the surrounding gasses.

CONVINCED, that experiments and observations on the spontaneous analysis of organic substances must be very instructive, and tend to illustrate their nature, I have always endeavoured to impress on the hearers of my chemical lectures, how great must be the influence of the elastic fluids, that surround putrescent bodies, either in accelerating or retarding this natural process. This also has determined me, to investigate the subject with great care; and I venture to lay before the public the results, as constituting a series of experiments, that may tend to the advancement of the science.

Experiments on the subject.

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* Ann. de Chim. vol. LXXIII, p. 300. Translated from Gehlen's Journal, vol. VII, No. 2.

It will be proper to premise:

The substance employed.

1st, That I always employed the same kind of flesh, namely beef, in order to be more certain, that the differences observed arose only from the action of the different elastic fluids: that the flesh used at one time was taken not only from the same animal, but from the same muscle; that it contained no fat, but muscular fibre alone; and that the pieces were of equal size, cut in parallelopipedons, and proportional to the vessels:

2dly, That I took only the flesh of an animal that had been dead two hours:

The gasses.

3dly, That I used only the last portions of gas evolved, in order that it might not be mixed with any of the air in the receivers; and that I employed the gasses soon after they had been prepared. The atmospheric air I took from a very airy garden.

Place of the experiments.

Temperature.

4thly, The vessels were placed in a room, into which the sun never entered. The windows also, facing the north, were very small, to avoid the action of light, which I purpose to examine on a future occasion. The temperature of this room is cool in summer; and in winter high enough to keep water from freezing. If, however, there were any reason to be apprehensive of frost in the night, I removed the vessels into my lecture-room, which joined it.

The gas employed in three modes.

I took three modes of enclosing the flesh in the gasses; and, to avoid repetition, I shall denote them by the following expressions: 1st, over water: 2d, over mercury: 3d, in an empty bottle.

Manner of conducting the experiments over water:

1. For the experiments over water, cylindrical jars were taken, containing from 92 to 98 cubic inches, Paris measure, and filled over a pneumatic trough. Pieces of meat 3¼ inches long, 1 inch broad, and ¾ of an inch thick, were then introduced into them. These were suspended from two cross pieces of brass, having a point ½ an inch long to fix the meat on, and supported by a stem of the same metal, with two other cross pieces for a foot. When this stand, with the meat thus disposed, had been introduced through the water into the jar, a plate was passed into the trough, and the jar placed upon it, so that it might be removed from the trough, and set on a table. When the water rose in

the jar, which is the case when the temperature diminishes, or the gas is absorbed, care was taken to add water, in order to prevent the introduction of atmospheric air. In this way the contact of atmospheric air is avoided, but it is attended with the inconvenience of wetting the whole of the surface of the meat, and of the gas and meat being in contact with the water in the jar.

in a bottle stopped with a cork:

2. For the experiments in an empty vessel, that is to say, containing neither water nor mercury, bottles resembling those for wine were taken, but with wider mouths. These were filled with gas over the pneumatic trough, and let stand till they were well drained. The meat was then introduced, keeping the mouth of the bottle above the water, corked as quickly as possible, and the cork well luted with paper and glue, or the neck of the bottle placed in water. On turning up the bottle, the piece of meat falls to the bottom, to which it commonly adheres in consequence of its moisture. By this mode we avoid wetting the meat; and neither the meat nor the gas is in contact with so large a quantity of aqueous vapour: but it has this inconvenience, the meat has been in contact with atmospheric air, and a little is always introduced with it, when the cork is put into the bottle. This process cannot be employed with nitrous gas.

and over mercury.

3. For the experiments over mercury, small jars, of 3 or 4 cubic inches only, were filled with the gas to be subjected to experiment; and pieces of meat, an inch long, ½ an inch broad, and 2 lines thick, were then passed through the mercury with the fingers, and introduced into the jars. By this process the contact of atmospheric air is avoided; as well as of aqueous vapour, when the gasses have been procured over mercury. But these experiments I could execute only on a small scale, as I had not a sufficient quantity of mercury for any thing more.

A little water, or mercury, should always be left in the jars, in order that the air, when expanding, may not get out of them. In the experiments over mercury it affords the advantages of preventing the vessel from being overturned.

First series of experiments, over water, begun the 25th of March, and ended the 4th of April, 1808.

1st set of experiments over water.

Temperature.

The temperature of the air out of doors was always between 8° of Reaum. [50° F.] in the shade, as for instance on the 30th of March in the morning, and 5° R. [43·25° F.], as on the 25th of March at noon.

1. *Oxigen gas.*

Exp. 1. With oxigen gas from nitrate of potash.

The gas was obtained from nitrate of potash.

1st day. March the 25th. The meat is become evidently redder, and even of a finer red than in nitrous gas.

2d and 3d days. The red colour has diminished, but the meat has still a fresh appearance.

4th and 5th days. The same.

6th and 7th days. The redness has gradually diminished; and the meat is moister than that in nitrous or in hidrogen gas.

8th day. The meat grows damp; it begins to get livid; and little hemispherical and almost transparent drops appear standing separate from each other on its surface.

9th day. The drops become more numerous, and grow gradually opake and whitish; so that the meat looks as if covered with small pox.

10th and 11th days. Putrefaction makes evident progress, the meat grows flabby, the moisture increases, and the surface dissolves; yet the drops may still be distinguished at the surface of the liquid, that covers the meat entirely.

On the evening of the 11th day the meat was taken out, after the jar had been replaced in the trough. It emitted a putrid smell, somewhat alliaceous, having some resemblance to that of phosphuretted hidrogen gas.

The meat continued to putrefy in the open air, as if it had been constantly exposed to it.

The oxigen gas was not much diminished in bulk. It no longer set fire to a candle, but a candle still burned in it for a moment with a little brilliancy.

2. *Hidrogen gas.*

Exp. 2. With hidrogen gas

This was prepared by dissolving zinc in sulphuric acid diluted with water.

from zinc and sulphuric acid.

1st day. March the 25th. In the course of a few hours the meat had acquired a dirtier colour; and at length became of a blackish brown, like meat that has been smoked after having been salted without the addition of nitre.

From the 2d to the 11th day. No change in the external appearance of the meat; particularly it has grown neither flabby nor moist: its firmness appears even to have increased; and it seems more hard and dry. It has lost its redness more and more, and is become browner than meat exposed to carbonic acid.

On the evening of the 11th day the meat was taken out. It had no sign of putrescency, and not the least offensive smell: the most that could be said was, that it smelt slightly sour. On exposure to the air it did not putrefy, but became dry: a few small white specks of mouldiness, however, were perceivable on its surface.

On turning up the jar, and applying a lighted candle to it, the gas took fire.

3. *Carbonic acid gas.*

Exp. 3. With carbonic acid expelled from chalk by nitric acid.

This was prepared by dissolving chalk in nitric acid.

1st day. March the 25th. In the course of a few hours the meat had assumed a dirty colour, and afterward became brown; but it remained of a lighter colour than that in hidrogen gas.

From the 2d to the 11th day. At first it appeared to grow more livid and flabby, but after a few days no change was observed. The surface did not appear to get moist. The carbonic acid gas, used in the experiment, is absorbed by the water; and it even appears to be more readily absorbed than when pure, so that fresh gas must be added every day, to prevent the water from touching the meat.

On the evening of the 11th day the meat was taken out. It had no marks of putrescency; resembled meat that had been dressed; was flexible, without being moist or viscous; and had a slight acidulous smell, nearly like that of yeast turning sour. On exposure to the air it did not grow putrid, but dried, and its surface became covered with little white specks.

4. *Nitrous gas.*

Exp. 4. With nitrous gas from nitric acid and copper.

Procured from nitric acid by means of copper, and received over water.

1st. day. March the 25th. The meat became of a much finer red than in atmospheric air; and for the first few hours it could not be distinguished from that in oxigen gas.

2d and 3d days. No change.

4th, 5th, and 6th days. The fine red colour diminished a little, but it still remained very lively.

From the 7th to the 11th day. No change was observable, except that the meat appeared to grow a little moist; but it did not liquefy at all at the surface, and its firmness even seemed to increase.

On the evening of the 11th day the meat was taken out. It had a fine red colour, was firm, and had no smell, not even of nitrous gas. It lost its redness when exposed to the air, in the course of a few hours; became brown; and dried much quicker than the meat of the two preceding experiments. Its surface did not become covered with white specks.

The gas, exposed to the test of oxigen gas, produced a quantity of red vapour, diminished greatly in bulk, and appeared not to differ perceptibly from common nitrous gas.

Second series of experiments, from the 5th of April to the 10th of June.

Second series of experiments.

Temperature.

The temperature of the air out of doors was 2·5° [37·75° F.] on the morning of the 18th of April, and 23·3° [84·4° F.] on the 17th of May at noon.

That of the room was between 7° and 20° [47·75° and 77° F.].

Meat used.

The meat employed in the following experiments was of a paler colour, and appeared to have come from a younger animal.

5. *Oxigen from red oxide of mercury. Experiment over mercury.*

Exp. 5. Oxigen from red oxide of mercury, over mercury.

1st day. April the 5th. The meat became redder.

2d and 3 days. No perceptible change.

4th and 5th days. The colour grew paler.

From the 6th to the 8th day. The colour was destroyed, and the meat had the appearance of having been washed.

9th day. The drops appeared at the surface as in the first experiment.

18th day. The drops, became opake, appeared like the eruption of small pox. The meat preserved its firmness without liquefying, though the temperature was higher than in the former experiments.

From the 19th to the 21st day, May the 25th. Visible signs of putrescency were observed on the surface, the little drops ran into each other, and the surface became blackish.

An accident having overturned the jar, the gas escaped, and diffused such a strong stench throughout the house, that we were obliged to perfume it strongly, to get rid of the smell.

6. *Oxigen from nitrate of potash, in an empty bottle, stopped with a cork.*

Exp. 6. Oxigen from nitrate of potash in a corked bottle.

1st, 2d, and 3d day. The meat did not grow redder.

4th day. It grew pale.

5th to the 51st. No little drops were observed, but the meat gradully grew pale, became putrid, and liquefied at the surface. At length a considerable quantity of liquid, of a bad colour, was formed, and flowed down into the neck of the bottle.

The meat was covered with moisture. Its smell was not so strong as that of the preceding piece putrefied in oxigen gas, and of a different kind.

7. *Atmospheric air, over mercury.*

Exp. 7. Atmospheric air over mercury.

1st and 2d days. April the 5th and 6th. No remarkable change.

4th day. The meat is become very pale; paler than in the oxigen gas.

5th to the 51st. The little drops of liquid were not perceived. From the 8th day it became covered with moisture, and liquefied at the surface, but less than that in oxigen gas; and at the close it appeared less black than in pure oxigen gas, On taking it out of the receiver, its stench was not so powerful, and it appeared redder when cut.

8. *Pure hidrogen gas from the vapour of water passed over redhot iron. Over mercury.*

Exp. 8. Hidrogen from water by redhot iron, over mercury.

1st day. April the 5th. The meat became of a crimson red.

From the 2d to the 51st. No change was observed, except that the meat became a little brown; but it did not acquire a livid hue. It is remarkable, that this meat remained reddish, and preserved an appearance of freshness, while pieces in oxigen gas, and in atmospheric air, grew pale. When taken out of the jar, it had no smell. The gas examined at the end of the experiment rendered lime-water turbid.

9. *Pure hidrogen gas in a corked bottle.*

Exp. 9. Hidrogen gas in a corked bottle.

1st to the 51st day. The meat scarcely grew brown at all, but preserved its colour, only appearing a little moist. When taken out the 51st day it had no bad smell, but smelt like smoked meat.

The gas, examined by the test of nitrous gas, gave no sensible diminution; it rendered lime-water a little turbid, and afterward burned vividly.

10. *Pure carbonic acid, from the calcination of chalk. Over mercury.*

Exp. 10. Carbonic acid, expelled from chalk by heat, over mercury.

1st day. The meat became crimsoned, as in hidrogen.

2d to the 11th. No sensible change, and the meat looks very fresh.

13th to the 22d day. It grew paler.

51st day. The meat was uniformly pale, looked as if it had been dressed, and appeared nearly of the same firmness.

It was neither moist nor viscous; and had not the least smell, or any other sign of putridity.

The gas was absorbed by lime, except a trifling residuum, that did not amount to 0·01.

Exp. 11. The former repeated in corked bottles.

11. If this experiment be repeated in corked bottles, and the meat be enclosed in one while the gas is hot, and in the other not till it has grown cold; it will be found, that the

meat in the cold gas will have kept well till the 16th day, but will have acquired an unpleasant smell; while that in the hot gas stinks on the 30th day, and is completely spoiled on the 60th.

12. *Nitrous gas, over mercury.*

Exp 12. Nitrous gas over mercury.

1st day. The meat became redder.

51st day. The meat has retained its fine colour, and is firm. The liquid, that has flowed from it, has assumed a fine red colour, and deposited a small portion of white matter, resemblimg fat, though the meat contained none.

67th day. June the 10th. The meat has retained its fine colour still, on which account I did not take it out, that I might see how long a time was necessary to effect its decomposition.

Third series.

Third series.

The temperature was the same as in the preceding series.

13. *Oxigen, over water, the jar contained only* 28·5 *cubic inches.*

Exp. 13. Oxigen gas over water.

1st day. The meat became of a fine red.

2d, 3d, and 4th. The meat retained its colour, and did not appear to putrefy.

6th day. Little transparent drops were observed; which increased in number and size on the 7th day, and grew turbid and red on the 8th.

9th day. The putrefaction was evident over all the surface, which began to liquefy. The gas diminished greatly in bulk. There is no doubt but the increase of temperature is the cause of the speedy putrefaction.

10th day. The gas, measured in the gasometer, had diminished 7 cubic inches. Being placed in contact with milk of lime, it diminished 6·5 cubic inches more. Supposing, that the 7 inches absorbed by water were carbonic acid, 13·5 inches of oxigen were expended, which must have formed 13·75 inches of carbonic acid gas*.

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* The carbonic acid gas, as appears from the best recent experiments, must have been precisely the same in bulk as the oxigen gas expended. C.

The 15 inches remaining having been tested with nitrous gas, I found that they contained 5·4 of nitrogen, and 9·6 of oxigen. Thus the 28·5 inches of oxigen gas have been employed in

13·5 carbonic acid,
5·4 nitrogen,
9·6 oxigen.

14. *Atmospheric air.*

Exp 14. Atmospheric air, over water.

The meat putrefied, and was decomposed on the 48th day. The water rose considerably, and absorbed 21 cubic inches out of the 96, that the jar contained. The experiment having been deranged, I could not continue my observations.

15. *Pure hidrogen gas.*

Exp. 15. Hidrogen gas.

1st day. The meat became of a poppy colour.

4th day. No change, except that the meat appeared dried.

6th day. Some mouldiness observed, that increased on the 7th day.

From the 8th to the 41st day no farther change was observed, except that about the 20th the mouldiness had disappeared. The meat resembled beef salted without nitre and smoked. It had not the least bad smell.

The gas, tested with lime-water, did not render it turbid. It burned with force and energy.

From these experiments it appears how necessary it is, to repeat them separately, in order to obtain some certain results. It appears however, that we may draw from them the following conclusions.

General deductions.

1. That hidrogen preserves, and even increases the firmness of meat, by drying it. That oxigen on the contrary diminishes this firmness, rendering the meat flaccid and moist. It is remarkable, that hidrogen preserves this firmness even over water, when the gas is saturated with moisture.

2. That meat is changed and liquefied much more speedily in oxigen, when it contains nitrogen, as in atmos-

pheric air, and in the gas from nitrate of potash, than when the gas is pure.

3. That nitrous gas resists putrefaction most powerfully; next to which comes hidrogen, and then carbonic acid.

4. That meat does not change so soon in oxigen gas, as in atmospheric air; but, when putrefaction has commenced, it proceeds with more energy than in atmospheric air, and diffuses a much more offensive smell.

5. That the colour of meat gets browner in hidrogen, and lighter in oxigen and in nitrogen.

6. That neither hidrogen, nitrous gas, nor carbonic acid, appears to undergo any sensible alteration from the meat included in it.

7. That oxigen gas, whether pure, or mixed with nitrogen, is converted into carbonic acid.

8. That part of the oxigen gas retains its properties, as in other combustions.

9. That, during the putrefaction of the meat in oxigen gas, nitrogen is produced; which nitrogen must be evolved from the meat, or oxigen has been converted into nitrogen.

10. When meat begins to grow putrid in hidrogen, it appears, that carbonic acid is evolved; but this does not take place, as long as the meat keeps without spoiling.

11. That on meat in oxigen gas little drops of water are formed, which resemble the eruption of small pox.

Continuation of the inquiry.

A continuation of my inquiries will be instituted for the purpose of verifying the facts I have announced; and partilarly of ascertaining, whether the carbonic acid gas, found mixed with the hidrogen, existed in the meat; and of investigating the influence of light, and the luminous properties of stinking meat.

III.

Continuation of Mr. HILDEBRAND'S *Paper on the Action of Gasses on dead Animal Flesh.*

Meat left a long time in nitrous gas.

1. I Have said in my 12th experiment, that, finding the meat not altered after having remained 57 days in nitrous

gas, I resolved to leave it there a longer time. Accordingly I did not take it out till the 25th of August, when it had remained in contact with the gas for 134 days. In the first months the temperature was between 7° and 20° [47·75° and 77° F.], and in the last months it was between 12° and 23° [59° and 83·75° F.] The temperature therefore was much higher, than was necessary to favour the decomposition of the meat, yet it retained a fine red and fresh colour. The liquor however, which had such a fine red colour, lost this in some measure. Having taken the meat out of the nitrous gas, to examine it more carefully, I observed, that, wherever it had touched the sides of the glass, it had become yellowish. (This I presume was owing to the contact of the glass diminishing the action of the gas.) In other respects, it was still of a fine red, had a good degree of firmness, and did not stink in the least; but it had a slight smell of nitric acid, with which a little of a peculiar smell was observed. Thus we see, that a longer time and a higher temperature produced changes, which did not take place in a shorter time and at a lower temperature.

Temperature.

Results.

The white sediment, mentioned in my first experiment, was found on examination to be coagulated fibrin. When agitated in water, it exhibited itself in the form of the little strings, that remain after the washing of the coagulum of blood. Boiling water did not dissolve, but hardened it.

Fibrin deposited.

The nitrous gas, that had been used for the experiment, when brought into contact with atmospheric air produced much red vapour; and the diminution of bulk was as great, as at the moment of its preparation.

The gas apparently unaltered.

II. Having remarked in experiments 8 and 9, that the meat, which had remained 51 days in hidrogen gas at a temperature from 7° to 20° [47·75° to 77° F.], rendered lime-water turbid, I instituted the two following experiments.

Carbonic acid produced in hidrogen gas.

16. *Hidrogen gas.*

I prepared this gas with zinc and dilute sulphuric acid, to obviate the objection, that the carbonic acid gas, contained in the hidrogen prepared by passing the vapour of water over a red-hot iron, might have originated from carburet of iron.

Exp. 16. Hidrogen gas from zinc and sulphuric acid, over mercury.

Temperature.

I enclosed the meat over mercury. The thermometer out of doors, from the 23d of July to the 14th of September, was between 8·5° and 26° [51° and 90·5° F.]; that in the room between 11° and 20° [56·75° and 77° F.]. The shutters of the room were closed, except when the meat was examined.

Results.

1st day. July the 26th. In a few minutes the meat became of a brown and livid hue.

2d day. The colour became again a little red.

22d day. August the 17th. It retained a pretty red and fresh appearance, without any mark of putridity.

34th day. Every thing was in the same state, and appeared to remain so till the 54th day.

State of the meat the 54th day.

Having taken the meat out on the 54th day, I found it as firm, as if it had been fresh; but it emitted an insupportable stench, differing however from that of meat putrefied in oxigen gas, or atmospheric air. Thus it appears, that organic matter can undergo a kind of alteration, and diffuse volatile principles occasioning noisome smells, without losing its cohesion, as in putrefaction properly so called.

Part of the gas converted into carbonic acid.

The bulk of the gas in this experiment was diminished only according to the variation of the temperature. At 14° [63·5° F.] it was 4·75 cubic inches, French measure. When I passed it through lime-water, it rendered it very turbid; I therefore agitated it with milk of lime, and, having measured it afresh, it was reduced to 3 cubic inches, so that 1·75 cubic inch of carbonic acid had been formed.

17. *Hidrogen gas.*

Exp. 17. Hidrogen gas, over water.

I placed a piece of meat in some of the same hidrogen gas; and the apparatus was kept with the preceding, and under precisely the same circumstances; except that the jar was over water, and contained 52 cubic inches.

Results.

1st day. July the 26th. The meat experienced the same change, as in the preceding experiment.

2d day. The colour appeared a little redder than in the experiment over mercury.

22d day. The meat looked red and fresh, and seemed drier than in the experiment over mercury. It appeared wrinkled.

The water has risen considerably in the jar, and rises every day.

54th day. The meat was of a fine red, but no longer wrinkled: it appeared drier, and resembled smoked meat. Taken out of the gas, it was as hard as smoked meat, but it diffused a horrible smell.

State of the meat on the 54th day.

As the temperature was 20° [77° F.] when the experiment began, and was now but 14° [63·5° F.], the quantity of 52 cub. in. should be reduced, according to Gay-Lussac, 0·14; and, according to Schmidt, 0·17: but there remained only 36 cub. in. of gas; consequently 15·83, or 15·86, were absorbed.

Diminution of the gas.

The gas, placed in contact with lime-water, and with milk of lime, diminished only a quarter of a cubic inch more; so that the total absorption was 16·08, or 16·11.

If the water and the milk of lime did not absorb as much gas in this experiment, as the milk of lime did after the experiment over mercury (for the ratio of 4·75 : 1·75 would give for the 52 cub. in., $19\frac{3}{19}$), it is nevertheless evident, that in both the experiments there was a considerable formation of carbonic acid, amounting to more than a fourth of the bulk of the hidrogen employed.

A considerable portion of carbonic acid formed,

Though distilled water may contain carbonic acid, we cannot surely ascribe to it this great quantity of gas; particularly as it was continually rising in the vessel, so that it was absorbing the gas, and not giving it out. Besides, the experiment over mercury removes every doubt. The formation of carbonic acid therefore was owing to the decomposition of the meat: for hidrogen alone would remain for years over water or mercury, without being decomposed in any way. But though the gas, that remained after the separation of the carbonic acid, was hidrogen; it is equally certain, that part of the hidrogen had disappeared: for, had not this been the case, the bulk of the gas should not have been altered in the 17th experiment, and in the 16th it should have been considerably increased.

by the decomposition of the meat;

and part of the hidrogen lost,

As it is not probable, that this hidrogen combined with the meat in decomposition; since it appears, that meat gives out an hidrogenious vapour when it changes; it would follow, that the hidrogen, which disappeared, was combin-

apparently combined with or forming the carbonic acid.

ed in the mixture of carbonic acid gas, which I cannot explain.

IV.

On the Nonexistence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. In a Letter to ALEXANDER MARCET, *M. D., F. R. S. from* WILLIAM HYDE WOLLASTON, *M. D., Sec. R. S.*[*]

MY DEAR SIR,

Nonexistence of sugar in diabetic blood.

IN reply to your inquiry respecting my experiments upon the nonexistence of sugar in the serum of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

Attempt to detect it.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the diabetes (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

Farther experiments

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it; for, as I soon after[†] relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits, which are generally interesting to the Royal

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[*] Phil. Trans. for 1811, p 96.

[†] 1800.

Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments; requesting that you will in return state, more fully than you have hitherto done, the result of that farther step in the inquiry, which you took at my suggestion; and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

Dr. Rollo and Mr. Cruickshank had no opportunity of determining the point directly,

Although Dr. Rollo had been assisted in the chemical part of his inquiry by the well known talents of Mr. Cruickshank, it appears, that they “had not been so fortunate as to obtain a sufficient quantity of serum for chemical experiment[*];” and were unable fully to satisfy themselves by the taste, or by other means which they could employ, concerning the existence or nonexistence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence.

but attempted it indirectly.

For the purpose of forming some judgment on this question, Mr. Cruickshank made trial of the quantities of oxalic acid, that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons.

Their method objectionable.

This method, it is evident, is liable to a twofold objection; first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed; and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result.

Method employed by the author.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient; for, though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

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[*] Rollo on Diabetes, p. 408.

The albumen in serum not completely coagulated by heat,

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exuded from it was still found to contain a portion of albumen dissolved in it; and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

unless a small portion of dilute acid was previously added.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation*. To six drams of serum I added half a dram of muriatic acid previously diluted with one dram and a half of water, and immersed the phial containing them in boiling water during four minutes. The coagulation was thus rendered complete. In the course of a few hours a dram or more of water exudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

Salt in the serum.

Effect of the presence of sugar.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness, that appears after evaporation: occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

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* I presumed, that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralising it.

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness, and disposition to deliquesce are of course more manifest than with smaller quantities*.

Sugar from diabetic urine had the same effects.

As I was aware, that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

Farther test of sugar.

As a farther test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts, that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop; and, if farther heat be applied, it becomes black in proportion to the quantity of sugar present.

Difficulties in experiments.

Such are the appearances, when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction, at the time when these experiments were conducted; and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances, which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself as to the phenomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

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* In Plate V, fig. 6, are represented the degrees of blackness of the drop occasioned by adding one, two, three, and four grains of sugar to six drachms of serum.

The

Trial with diabetic blood that had been dried.

No sugar.

The first which I examined was a portion of blood that had been taken from a person, whose urine had been analysed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be extracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

2d experiment.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

3d experiment.

In a third instance, I had some serum from the blood of a person, whose urine had been tasted, and found "*very sweet.*" (I had no opportunity of procuring any of this urine for analysis). After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic acid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

4th experiment.

An equivocal appearance.

I had also a fourth opportunity of examining serum of a person, whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat as sugar is: and its refractive power* was too great for that of sugar.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

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* The method by which this was tried has since that time been described in the Phil. Trans. for 1802. See Journal, vol. IV, p. 89.

5th experiment.

In the next experiment I added half a dram of the urine of the same person to six drams of the serum, and, with a due proportion of diluted muriatic acid, coagulated as before. Although the quantity of extract added did not exceed $\frac{3}{16}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

No sugar in serum.

The result of these trials was such, as to satisfy me, that the serum in this instance contained no perceptible quantity of sugar; or, at least, that the water separable from the coagulated serum did not contain one thirtieth part of that proportion, which I had found in the urine of the same person.

The sugar formed in the kidneys,

or conveyed to them directly from the stomach.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood vessels. That some such channel does exist, Dr. Darwin* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it was conveyed to be the absorbent system, upon the supposition, that they might admit of a retrograde motion of their contents.

Dr. Darwin's attempt to prove this by nitre.

The author's by prussiate of potash.

Without adopting the theory of Dr. Darwin, it did appear to me, that the fact deserved to be ascertained by some test more decisive than nitre, and I conceived, that, if prussiate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found, that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

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* Account of the retograde motion of the absorbent vessels, by Charles Darwin.

Experiment.

After a few previous trials of the period, when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person, about thirty-four years of age, was induced to take a dose corresponding to three grains and a half of the dry salt, and to repeat it every hour to the third time. The urine, being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect: and as I thought, that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern, that any degree of blueness was occasioned by it.

The experiment repeated.

This experiment, having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

No prussiate found in the saliva,

Being desirous of ascertaining, whether the prussiate could be discovered in any other secretions, I have repeatedly examined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

or aqueous

I have also, during a severe cold, accompanied with profuse

fuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

discharge from the nose.

Trial of the serum from a blister.

It was nearly in this state, that I left the inquiry at the period I have mentioned; and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum, that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance, by which substances may reach the bladder.

Retrograde action of the absorbents improbable.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Secretion generally effected by electricity.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope, that we might from this source derive some explanation of such phenomena. But though I have referred[*] secretion in general to the agency of the electric power with which the nerves appeared to be indued, and am thereby reconciled to the secretion of acid urine from blood that is known to be alkaline, which before that time seemed highly paradoxical; and although the transfer of the prussiate of potash, of sugar, or of other substances, may equally be effected by the same power as acting cause; still the channel through which they are conveyed remains to be discovered by direct experiment.

The channel still to be discovered.

I have, indeed, conjectured, that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu*; but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject.

I remain, dear sir, with great regard,
Yours very truly,

January 1, 1811. W. H. WOLLASTON.

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[*] Philosophical Magazine for June 1809.

V.

Dr. MARCET'S *Reply to Dr.* WOLLASTON, *on the same Subject.*

Russell Square, January 8, 1811.

MY DEAR SIR,

The hypothesis of the presence of sugar in diabetic blood specious.

I Am much gratified to find, that you have at last been induced to communicate to the Royal Society your curious inquiry respecting the state of the blood in diabetes. I was anxious, that the specious hypothesis of the presence of sugar in diabetic blood, which had been sanctioned by the authority of Dr. Rollo and Mr. Cruickshank, and which I had myself urged in support of their theory, fourteen years ago, in an inaugural publication, should no longer obtain an undue weight among physiological inquirers.

Attempt to ascertain whether prussiate of potash, taken internally, be present in any other secretion beside urine.

With regard to the experiments which I tried at your request some years ago, with a view to ascertain whether prussiate of potash taken into the stomach, and found to exist in the urine, could also be detected in other secretions, I find, on referring to my memorandums, the following particulars, which I shall transcribe verbatim.

" August 19, 1807. Having heard from Dr. Wollaston, that prussiate of potash could be taken into the stomach with perfect safety, and that its presence could afterward be discovered in the urine, but not in the serum; and being invited by him to follow up this inquiry, with a view to connect it with the theory of diabetes, I tried the following experiments.

Exp. 1. Prussiate of potash may be taken in considerable doses.

Exp. 1. " After having satisfied myself, by trials made by some medical gentlemen upon themselves, that considerable doses of prussiate of potash might be taken without the least inconvenience, I gave to a young woman, labouring under diabetes mellitus, five grains of prussiate of potash dissolved in water, and this was repeated every hour, till she had taken thirteen or fourteen such doses. After the fifth dose, her urine, by the addition of a drop or two of a solution of sulphate of iron, turned blue instantly. At this period of the experiment, a blister was applied to her stomach, and after a few hours, while still taking the prussiate of potash, and while the urine strongly indicated its presence, the blister was cut, and the serum collected. This serous fluid being, in the same manner as the urine, subjected to the action of a solution of sulphate of iron, did not suffer any change of colour in the least indicative of the presence of prussic acid. Yet the urine still remained capable of imparting a blue colour to solution of iron, fifteen hours after taking the last dose of the prussiate of potash.

None in the serum from a blister.

Exp. 2. "The same person being soon afterward put upon a course of ferruginous medicines, and having taken considerable quantities of sulphate of iron, an idea naturally occurred to me, that the phenomenon might perhaps be reversed; but upon adding prussiate of potash to the urine, no vestige of iron could be discovered, and the same attempt was repeated several times with the same negative result.

Exp. 2. Sulphate of iron taken.

No iron in the urine.

Exp. 3. "Dec. 2, 1807. The fluid obtained by means of a blister (as in Experiment 1.) being not immediately derived from the circulation, since it may be considered as the product of a secretion, I was desirous of repeating Dr. Wollaston's experiment on the serum itself, under circumstances of impregnation similar to those, in which the serum of the blister was examined.

Exp. 3.

"For this purpose, a young woman, after taking, in divided doses, about a dram of prussiate of potash in the course of twelve hours, lost some blood by cupping, an operation which had been ordered for a local complaint under which she laboured. The serum having been allowed to separate, and a little nitric acid having been added to it, not the least vestige of prussic acid appeared on applying the test of sulphate of iron, although the urine, made during the six hours which preceded and followed the cupping, was strongly impregnated with that acid, and struck a vivid blue upon adding the smallest quantity of iron."

Prussiate of potash detected in the urine, but not in the serum of the blood.

I have only to observe, in addition to these particulars, that the susceptibility, by which prussiate of potash is transmitted

Prussiate of potash not conveyed to

the bladder in all persons with the same facility.

mitted to the bladder, seems to vary in different individuals; for in five trials, made at Guy's Hospital in Nov. 1805, I failed of discovering any vestige of this salt in the urine of persons, who had taken it in quantities sufficient to produce its appearance in others. Three of these individuals, I should observe, were at the time under mercurial treatment; and an idea occurred to me, that, mercury having a great affinity for prussic acid, the presence of this metal in the system might prevent the effect in question. But, as in the two other failures no mercury was present, I cannot lay any stress upon this conjecture. It may be proper to mention, that, in the frequent trials which I have made with the prussiate of potash, no symptom or inconvenience whatever has ever occurred, which could be ascribed to this salt.

No inconvenience from taking it.

I remain ever,
My dear sir, with great esteem,
Yours sincerely,
ALEX. MARCET.

The channel of conveyance may be the arteries and lymphatics, to the exclusion of the veins.

P. S. While revising the proof of this sheet, it has been observed to me by some friends, and in particular by Dr. Henry of Manchester, and Dr. R. Pearson of London, that, in order to show distinctly that certain substances find their way to the bladder, without passing through the general circulation, it would be necessary to examine the arterial, as well as the venous blood, since it is not impossible, that the whole of the sugar in diabetes, or the prussiate of potash in the experiments above related, may be conveyed to the urinary organs by the arteries, without entering the venous system. According to this hypothesis, it may be conceived, that the same substances, when conveyed by the arteries to distant parts of the body, may return by the absorbent system, and might in this case be discovered in the thoracic duct. This view of the subject may deserve farther investigation; and I hope, that this curious question will soon be decided by appropriate experiments.

VI.

On the Algorithm of Imaginary Quantities. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Defective algorithm of imaginary quantities.

AS neither of the papers which you did me the honour to insert in your Journal, "On the Defective Algorithm of Imaginary Quantities*," has been noticed by any of your mathematical correspondents, I now propose to fulfil my promise, viz. "That if no answer appeared within three months, I would then, through the medium of your Journal, give my own explanation of the difficulty." I cannot, however, but regret, that, amongst the many able advocates for the introduction of these quantities into mathematical investigations, no one of them should have stepped forward to explain an anomaly attending such introduction, which seems to militate so much against the boasted certainty of results, deduced from mathematical investigations.

Results from imaginary quantities uncertain.

The defect pointed out in my former letters is, I believe, a new case, which has not been noticed by any of the writers on the subject; and, unless it can be satisfactorily answered, it is impossible to deny, that uncertainty and doubt must necessarily attend all results obtained through the medium of imaginary quantities. I am sorry, through the backwardness of your other correspondents, it has fallen to me to enter upon this subject; I will, however, do my best to explain the anomaly in question; at the same time I cannot but again repeat my regret, that it has not been taken up by some one more competent to the task.

Origin of imaginary expressions.

First then, let us consider the origin of these imaginary expressions, which may be traced to the very first principles of algebra. We are taught in multiplication, that $(+a)^2$, and $(-a)^2$, are both to be represented by a^2; and consequently, when the converse of this operation arises, or when we are required to extract the square root of a^2, it is either $+a$, or $-a$; but if it be required to extract the square root of $-a^2$, we must necessarily be stopped in our progress, for there being no previous convention, that $-a^2$ shall represent the square of any quantity; it follows, that, when such a case arises, we are not able to assign to it any particular root, and we are thus presented with the first and the most simple form of imaginary quantities. These expressions, however, though they have no definite value, yet, considered as mathematical symbols, ought to be subject to certain rules, as well as other symbols, which are the representatives of real quantities; the rules, however, which are laid down for operating in the latter case, frequently require certain modifications before they can be applied to the former, and most of the difficulties, which have occurred with regard to the algorithm of imaginaries, have arisen in making rules general, which were first intended to answer only in particular cases.

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* Vol. XXIX, p. 254, and XXX, p. 209.

Difficulties from applying generally what was intended for particular cases.

The ambiguity does not exist in certain cases.

We have seen above, that $\sqrt{a^2}=\pm a$; that is to say, it has an ambiguous root, which may be taken either at $+a$, or $-a$; but this ambiguity has not place, if we know how the quantity a^2 was generated, and have occasion afterwards to retrace the steps of our operation: we cannot, for instance, say, that $\sqrt{-a\times-a}=\sqrt{a^2}=\pm a$; or that $\sqrt{+a\times+a}=\sqrt{a^2}=\pm a$ for the square root of a in both these cases is determined; that is, when considered with regard to its generation, it has but one root; whereas its origin not been known, we must have prefixed the ambiguous sign to the root a, and for this obvious reason, that we know not, when a^2 is unconditionally assumed, whether it be the representative of $(+a)^2$, or of $(-a)^2$; these being both expressed by the same symbol a^2. In fact, there is no ambiguity in the extraction of the roots of quantities, except in those cases in which we are unacquainted with their generation; and in these it must necessarily arise, because we have agreed to represent the powers of different quantities by the same symbol.

Caused by expressing different quantities by one symbol.

This Illus.

There are, for instance, three different quantities, which, being cubed, give unity for the result; we say, therefore, that 1 has three roots; and, if we are simply requested to extract the cube root of 1, we may make it either 1 or $-\frac{1}{2}+\frac{1}{2}\sqrt{-3}$, or $-\frac{1}{2}-\frac{1}{2}\sqrt{-3}$; for the cube of each of these quantities is represented by 1. But if we are asked, what is the cube root of $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})^3$, we must not say, that $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})^3=1$, and therefore its root is also equal to 1; for, as in this case we know, that 1 is the representative of the cube of a particular quantity, its cube root must necessarily be that quantity, and no other.

trated by an example.

Hence it appears, that there is no ambiguity in extracting the roots of quantities, which are known to be generated from the constant multiplication of a given quantity by itself, whether this quantity be real or imaginary. But if, by the multiplication of two unequal factors, we arrive at any result, and have afterwards to extract the root of that quantity, there is then nothing in the nature of the case to limit the root. If, for example, we find, that the product $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})\times(-\frac{1}{2}-\frac{1}{2}\sqrt{-3})=1$; and we have afterwards to take the cube root of this product; there is nothing to indicate, that we ought to take one root in preference to another: whence it follows, that a quantity generated from the product of unequal factors has all the generality of a quantity unconditionally assumed; whereas that which is generated from the product of equal factors has not that generality, as it admits only of one root; while the other quantity, made up of unequal factors, has as many roots as there are units in the index of the power, the root of which is to be extracted.

No ambiguity when we know how the quantity was generated, unless it was the result of unequal factors.

Let us now see how far what has been said above will serve to explain the difficulty stated in my former letters, or rather in my last letter, to which I intend more particularly to direct the following remarks:

Application to the point in question.

The quantity which I proposed to square was the following,

$$\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}.$$

Now here the quantities under the cubic radicals are the two imaginary roots of the equation $x^3=1$; which, for the sake of simplicity, we will represent by a and b, the third root being 1. Then from the known theory of equations we have $1+a+b=0$, or $a+b=-1$;

also $1\times a\times b=ab=1$;

Answer to the question on imaginary quantities.

Now we have also $a^3=1$; $b^3=1$; but there is this difference with regard to these products represented by 1; viz. that the first has no limitation as to its roots, being the product of different factors, and is therefore as general as 1 unconditionally assumed; whereas the two latter have only particular roots, the first being a, and the second b, and they can have no other roots; if, therefore, in the operation of squaring, we have to take the roots of these quantities, we must pay particular attention to this circumstance; the want of which, in the numerical example in my former letter, is what gave rise to the incongruous result there deduced. Let us now square this literal expression by the common method, only observing, with regard to the products and powers, the rules above laid down:

$$\begin{array}{l} \sqrt[3]{a}+\sqrt[3]{b} \\ \sqrt[3]{a}+\sqrt[3]{b} \\ \hline \sqrt[3]{a^2}+\sqrt[3]{ab} \\ \qquad +\sqrt[3]{ab}+\sqrt[3]{b^2} \\ \\ \sqrt[3]{a^2}+2\sqrt[3]{ab}+\sqrt[3]{b^2} \\ \sqrt[3]{a^2}+2+\sqrt[3]{b^2} \end{array}$$

And that this is the true result, may be shown as follows. The proposed formula

$$\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}, \text{ or}$$

$$\sqrt[3]{a}+\sqrt[3]{b}$$

is the real root of the equation

$$x^3-3x=-1,$$

as deduced from Cardan's rule; and consequently the cube of that formula, *minus* three times itself, ought to be equal to -1. Let us therefore continue the operation, and see how far

Answer to the question on imaginary quantities.

far the result will agree with the required conditions: for this purpose we will repeat again the preceding square, viz.

$$\sqrt[3]{a^2} + 2 + \sqrt[3]{b^2} = (\sqrt[3]{a} + \sqrt[3]{b})^2$$
$$\sqrt[3]{a} + \sqrt[3]{b}$$

$$\sqrt[3]{a^3} + \sqrt[3]{a} + \sqrt[3]{ab^2}$$
$$+ \sqrt[3]{a^2 b} + 2\sqrt[3]{b} + \sqrt[3]{b^3}$$

$$a + 3\sqrt[3]{a} + 3\sqrt[3]{b} + b = (\sqrt[3]{a} + \sqrt[3]{b})^3$$

This result is evidently deduced according to the preceding rules, in which it is shown, that $\sqrt[3]{a^3} = a$, $\sqrt[3]{b^3} = b$, $\sqrt[3]{ab} = \sqrt[3]{1} = 1$, and consequently $\sqrt[3]{a^2 b} = \sqrt[3]{a b a} = \sqrt[3]{a}$ and $\sqrt[3]{a b^2} = \sqrt[3]{a b \cdot b} = \sqrt[3]{b}$

We have seen also, that, a and b being the imaginary roots of the equation $x^3 = 1$, $a + b = -1$; the above expression may therefore be written

$$x^3 = (\sqrt[3]{a} + \sqrt[3]{b}) = 3(\sqrt[3]{a} + \sqrt[3]{b}) - 1$$

also $-3x = 3(\sqrt[3]{a} + \sqrt[3]{b}) = 3(\sqrt[3]{a} + \sqrt[3]{b})$

whence $x^3 - 3x = -1$

as it ought to be; which shows, that our operations have been accurately performed.

It only remains, therefore, now to explain in what respect our numerical operation differs from that which has just been performed upon the literal symbols a and b. Now, upon a comparison of the two, we shall find them to be precisely the same, except, that in the former we have not retained the square form under the radicals; by which we lose sight of one of the particular circumstances, which ought to be kept in view throughout the operation. It happens in the example proposed, that the imaginary quantities under the radicals are powers of each other; as is the case with the roots of every binomial equation $x^n = 1$; that is, in our particular examples, $a^2 = b$ and $b^2 = a$: but this equality has not place when the quantities enter under a radical, and the

Answer to the question on imaginary quantities.

the reason of this restriction is obvious; because though $a^2 = b$; and consequently $a^3 = ab$, which are both represented by unity, or 1, yet the cube root of the former is a, and of the latter 1: as is evident from the preceding part of this paper, the first (1,) being produced from the cubing of a certain quantity a, and therefore having its cube root $= a$; and the other, being the product of two unequal factors, possesses all the generality of 1 unconditionally assumed.

Hence it appears, that there may be quantities equal to each other, which, when placed under the same radicals, lose their equality: the one of them being restricted to give a particular root, and the other a different root; and therefore, in such cases, although the quantities and the radicals are precisely the same, yet all equality between the two ceases, and they must be considered as totally dissimilar quantities.

This is exactly the case in the examples proposed, as may be made evident as follows:

We have seen, that $a^2 = b$, and $b^2 = a$; and if, in consequence of this equality, we make our first result, obtained from squaring, viz.

$$\begin{array}{l} \sqrt[3]{a^2} + 2 + \sqrt[3]{b^2} = \\ \sqrt[3]{b} + 2 + \sqrt[3]{a} \end{array}$$

as we did in the numerical example, we should find, that in continuing the operation, we should not arrive at the same result as in the former case; that is, by multiplying again, in order to cube the expression, we should have

$$\begin{array}{l} \sqrt[3]{b} + 2 + \sqrt[3]{a} \\ \sqrt[3]{a} + \sqrt[3]{b} \\ \hline \sqrt[3]{ab} + 2\sqrt[3]{a} + \sqrt[3]{a^2} \\ \qquad + \sqrt[3]{b^2} + 2\sqrt[3]{b} + \sqrt[3]{ab} \\ \hline \quad 1 + 3\sqrt[3]{a} + 3\sqrt[3]{b} + 1 = \\ \quad 3(\sqrt[3]{a} + \sqrt[3]{b}) + 2 \end{array}$$

which is different from the former result.

The difficulty therefore stated in both my former letters is thus explained, namely, that we have considered quantities as equal, because they are represented by the same symbols; whereas, in consequence of thus entering under radicals, and being restricted with regard to their roots, all equality between them ceases.

Method of guarding against such anomalies.

With regard to the proper criterion, by which such anomalies may be guarded against in other cases, there appears to be none more general than the following, viz.—that, in the involution of quantities under radicals, the operation should not be worked out at length, but indicated by the sign of the particular power, to which it is to be raised.

I have thus endeavoured to fulfil the promise which I made in my former letter; but how far what I have said may be considered as satisfactory must be left to others to decide. I shall only observe, that if any of your correspondents should perceive any defect in the reasoning employed in the preceding pages, I shall be extremely happy to see it corrected in a subsequent number of this Journal; if not, I may probably at some future time trouble you with a few other remarks on this subject.

Yours,

MATHEMATICUS.

VII.

Description of a Compensation Pendulum for a Clock: by Mr. ADAM REID, *of Green's End, Woolwich*[*].

SIR,

Half-second compensation pendulum.

YOU will have the goodness to lay before the Society of Arts &c. a half-second compensating pendulum of my invention; which is so simple in its construction, that it will be fully understood by viewing either the pendulum, or the drawing which accompanies it. I believe it to be new, and wishing it to be useful to the world, I have presumed to send it to the Society. I am, sir,

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[*] Trans. of the Soc of Arts, vol XXVIII, p. 230. Fifteen guineas were voted to Mr. Reid for this invention.

Your humble servant,
ADAM REID.

Reference to the Drawing of Mr. Reid's Compensation Pendulum, Pl. VI.

The pendulum described.

Fig. 1 and 2, Pl. VI, A B represent the steel rod extending through the whole. C the bob, supported upon the compensating cylinder of zinc D, which surrounds the rod A B, and rests upon the nut E of a screw tapped upon the end of the steel rod, to bring it to exact time; as this expands downwards by heat, the zinc expands upwards the same quantity; so that the bob always remains at the same distance from the point of suspension. Fig. 2, is a section to explain more clearly the thickness of the zinc tube D, and the form of the steel rod at *a*, where it passes through the bob, which is of the shape shown at L, that the rod or the bob may not turn round, when the nut E is turned to adjust it to time.

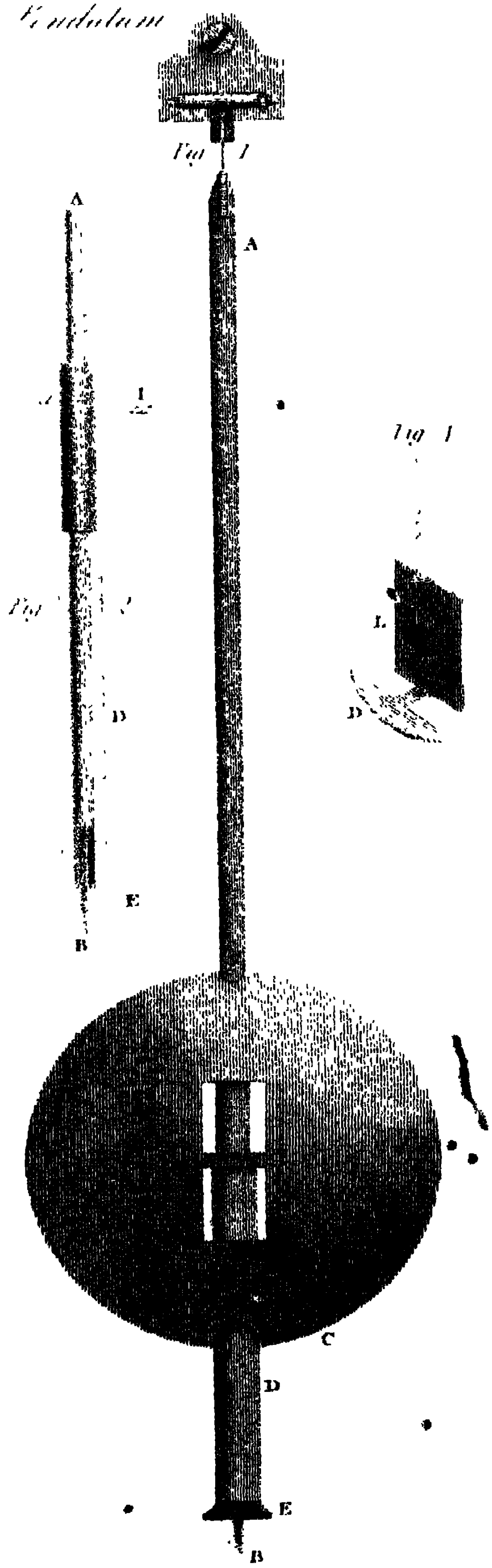
The following Method is to be employed to make the Compensating Pendnlum of Steel and Zinc.

Method of making it.

Procure a rod of forged blister-steel 52·7 inches long, ·27 diameter, heat it to a white heat, which will open the pores of the steel, and give it the smallest expansive power, that steel of this texture is possessed of. When cold straighten it with a mallet of wood on a wooden block, that no part may be condensed partially; which would be the case, if a hammer and anvil were used. Then cast a solid rod of zinc 12·5 inches long, ·68 diameter, with the lowest heat, that will fuse it; and pour it into a metal mould. This will give it the greatest density, consequently the greatest expansion, that zinc is possessed of. Then bore a hole through the centre of it longitudinally, that it may move freely on the steel rod, which has a nut and screw at the bottom end to regulate the clock to time; the bob, as shown in the engraving, rests on the upper end of the cylinder of zinc, and will continue in the same place, whatever expansion or contraction takes place, if the adjustment be correct.

If

Kent's Compensation Pendulum



Mr. Sparks's Noctuary.

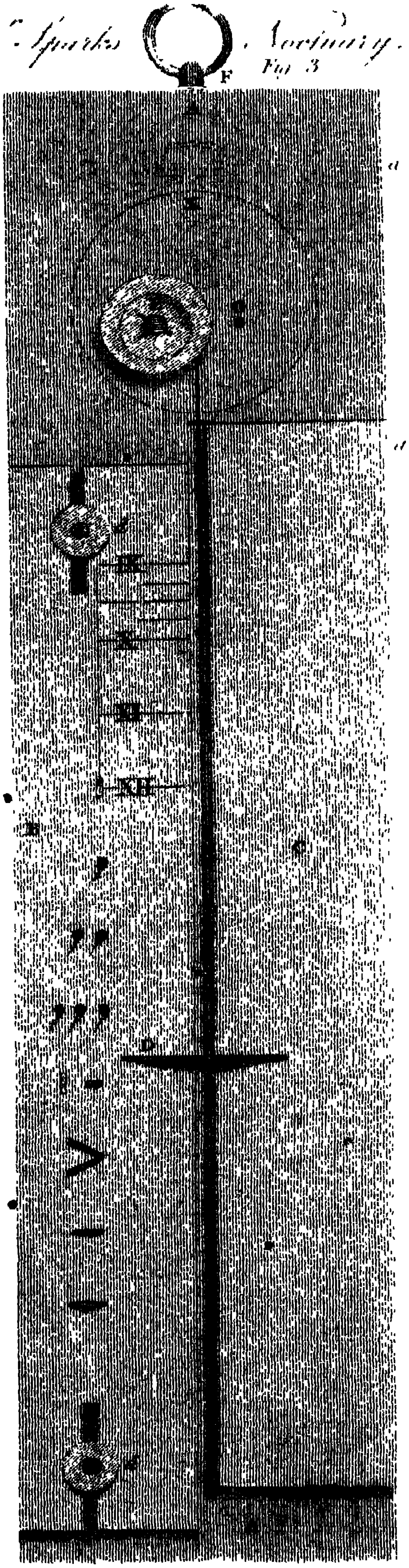


Fig. 2.

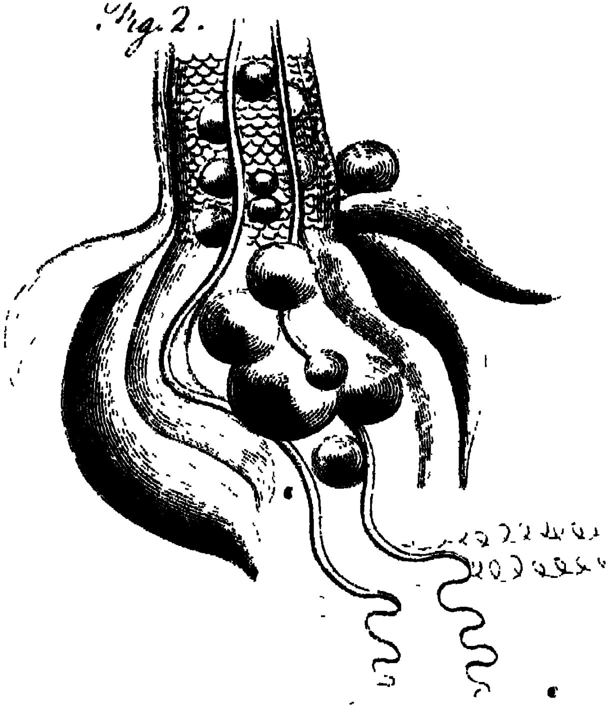
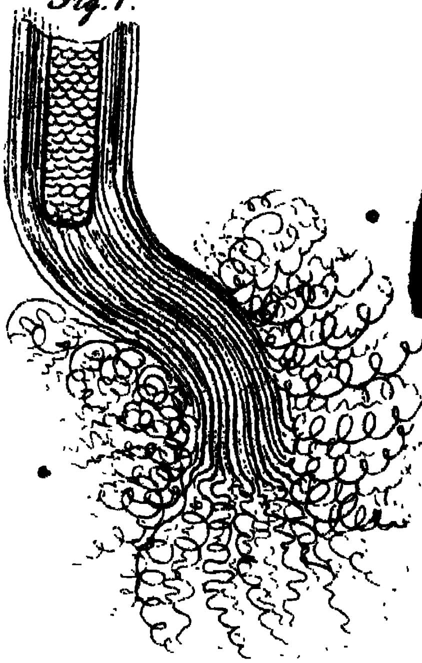


Fig. 1.



Fig

Fig. 3.

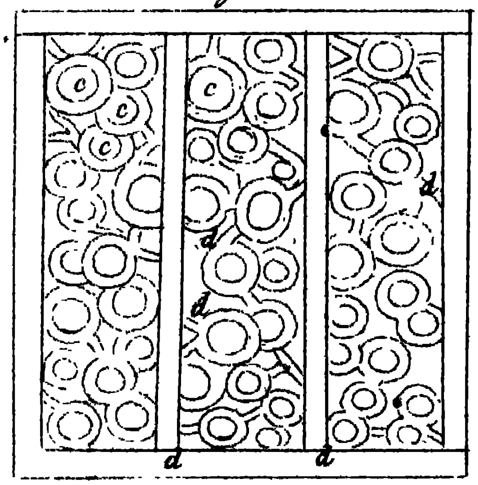
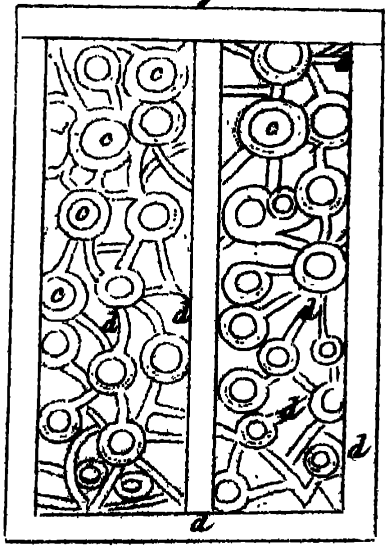
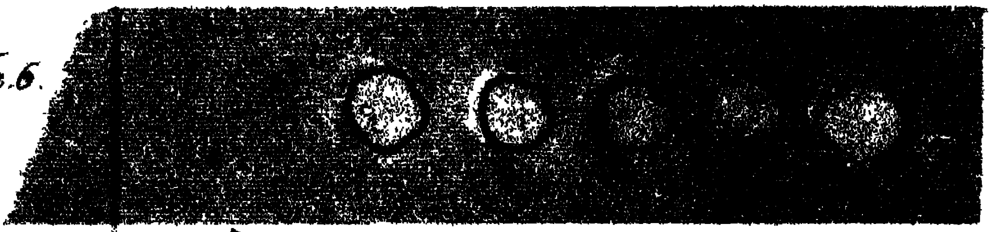


Fig. 4.



Sugar in Serum of Blood.

Fig. 6.



A

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AND

THE ARTS.

MARCH, 1812.

ARTICLE I.

On the different Sorts of Wood, with some Remarks on the Work of Du Thouars. *In a Letter from Mrs.* Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

On the treatise of Aubert du Thouars.

I Know not whether the account I have given of the mechanism of Botany has convinced my readers, or done justice to my subject; the latter is indeed impossible. I shall however (though thoroughly sensible of my deficiencies) renew the discussion as soon as the spring bestows a fresh return of her treasures for dissection. In the mean time I shall give a farther idea respecting the formation of *wood*, in some measure answering to the new Treatise on Vegetation, received from France, and written by Aubert du Thouars, celebrated for his studies in physiology, and who appears (like myself) to have taken nature for his guide, and left books for a future consideration; thinking it of greater consequence to ascertain a few absolute facts, than to collect a confused mass of uncertain details, that lead to no end.

The bud another kind of seed.

But the root and wood differ in many respects.

No returning vessels.

Each leaf has not a separate wood vessel.

Du Thouars agrees with me, that each bud is only another kind of seed, having its cotyledons the same, and wood vessels reaching from the bud to the root, and ending in a radicle, so that each tree may be esteemed (in this respect) a collection of plants, really distinct from each other, though joined under a double cover. But in my opinion he carries this idea much too far in giving the same appellation to both root and wood, for they certainly greatly differ in many respects, which he seems not to have noticed, beside the former having a double vessel, till it joins the radicle, while the wood vessel is single; as I have before described in my former account of this substance. He is also of my opinion respecting the supposed circulation of sap, which he appears persuaded does not exist, the liquor flowing to the terminating branches only, to give life to fresh matter, shooting from every extremity where the flow of sap will allow it to form; indeed, we are now so well acquainted with the different parts of the stem, that, if there were any returning vessels, they could not (I think) but be known. But he is undoubtedly mistaken in saying, that each leaf has a wood vessel, as well as the bud; for, if this was true, the stem must be as wide again. I have traced this part with such exact attention, as not easily to be mistaken; and have always found, that the set of wood vessels, after meandering all over the leaf, returns on the upper side of the stalk, and thus enters another leaf, that the same collection may serve many hundreds. It is not difficult to follow them, and is most convincing.

Where the flower bud is formed.

There is nothing that gives me more astonishment than the blindness of physiologists respecting the formation of the flower bud. Du Thouars, as well as Mr. K., thinks it is formed in the new wood, next the bark, and believes the bud is generated by the sudden mixture of the wood and bark, as if they did not run side by side throughout the plant; a strange mistake! I confess it is this discovery "of the shooting of the bud" I am most proud of, first because it is the foundation of many important facts, that lead to consequences of no little moment; secondly, because it is so very plain, so easily seen, that, strip off the bark, and a child would acknowledge and understand it; as the buds appear coming out from the interior of the wood in every part. But how many truths does this substantiate! It proves which is the vital part of a plant; that the impregnating line, which runs into the seed, is likewise found running next the pith in the stem; that this is the line of life, from which also all buds proceed; that the seed and bud are the same thing, at least differing in trifles only; and that they both owe their existence to this same vital part, one shooting in the flower, the other in the stem of a plant. In herbaceous plants this same line runs within the pith, but equally gives life to bud and seed; hence the truth of Linnæus's observation, when he gave such consequence to the pith of some plants.

Consequences deduced from this.

There is something so curious in the manner in which the bud is first united with the sap vessel that nourishes it, that, as I have not exactly shown the process, I shall give it here. I have described the manner in which the line of life first generates the bud by forming a knot on that line, and breaking the outward ends. Each end then becomes a bud, and it is the business of the wood to form a covered way for the passage of the bud to the exterior, which it does by bending some and raising other parts of its vessels, in the middle of which the bud passes to its cradle; but no sooner does the knot form on the line, than it becomes a signal for the root to send up a vessel for the nourishment of the new bud, and by the time it reaches its cradle, this vessel (loaded with sap) arrives at the same place, and fastens itself to the bud, affording it that nourishment the milky juice of the albumen could no longer give it. It is exactly the same process as that in the seed, where the milky albumen first supports, and is then succeeded by the nourishing vessels.

Manner in which the bud is first united with the sap-vessel that nourishes it.

I shall now give a curious proof, that the wood is the only part which carries the sap for the nourishment of the plant; and that the plant dies, if the wood vessel is lost. In tracing the various diseases of plants, especially in our kitchen vegetables, it occurred to me to examine thoroughly that which is called smut in potatoes; and compare it with the

The wood the only part that conveys the sap for nourishing the plant.

Diseases of plants.

Disorder of the brocoli.

The cause in the earth and not in the plant.

Ingredients of the sap not known.

the disorders which are found in the brocoli and turnip. In the brocoli the first apparent symptoms are a shrivelling and drooping in the stem of the plant; it for some time languishes like a consumptive patient, and then dies after a long illness. Curious to find what could be the cause of this disorder, and its effects on the interior of the plant, and why the plant ceased to gain that nourishment, which the earth seemed so fitted to give; I resolved to dissect the plant with care. To prepare myself for thoroughly understanding it, I took a brocoli plant growing in other ground, and perfectly healthy, dissecting and drawing it, as I give it at fig. 1, plate V. It will be seen how many radicles it has, how many wood vessels, both in the root and stem. I now laid open the diseased plant with the greatest care: but how excessive was my surprise to find, that almost all the wood vessels had disappeared, though the plant was twice the size of the healthy brocoli, and of a bulbous form in the root, see fig. 2; that in place of the wood vessels, I could see only tubercles filled with water, and that, instead of near a hundred radicles, two solitary ones, with their appropriate wood vessels, were all the plant had to bring it support. The bulbs of water were composed of a loose matter something like the pith of trees, but very large, and without one ligneous particle; in short the complaint appears like a dropsy in the plant. From various trials it was plain to be seen, that the defect arose from the earth; since if pease, beans, vetches, &c. were substituted for the brocoli, turnip, or potato, they would grow admirably, but the same disorder would equally attach to those vegetables just named, if placed in that earth. The cause then was the failure of suitable nourishment for those plants in that ground; and its consequences were the decay of the parts intended to bring that sort of sap, which the ground was not able to bestow. This shows also, that there is a great variety in the sap, though we are not able to discover it; indeed in our trials so much of the most important and delicate essence may evaporate, during the first attempt to ascertain the ingredients, that we cannot thoroughly trust to our knowing all that enters into the composition of this astonishing mixture. I cannot conceive that any thing but decay could cause the disappearance of the wood vessels; and am more confirmed in this idea, as I have always found that it is the constant consequence of the stoppage of the sap, whenever it takes place during the life of the plant. This I before showed, when I endeavoured to prove how mistaken physiologists were in supposing that part of the stem of the tree was void of sap, and was only filled in the newest part.

The ground too cold and moist.

On summing up the evidence I should conclude, that the ground is too cold and moist for these vegetables. On dissecting turnips and potatoes growing in the same ground, I find it exactly the same disorder as that in the brocoli, that is a total disappearance of the ligneous parts, both in stem and root. The tubercles of water mentioned in the brocoli, turnip, &c. are always full of stinking water, as the putridity is just beginning. May not the disorder of these plants in some measure serve to prove, with the rest of the facts already adduced in my former letters, that the sap is conveyed by the wood alone for the nourishment of the plant? I think the body of evidence I have at different times given now nearly amounts to certainty, that the sap runs in the wood alone, and does not circulate. This disorder must not be mistaken for that called the scab in potatoes, which is a malady that proceeds from a cryptogamian plant, first growing on the outside of the potato, in which insects fix and lay their eggs, to insure food to their hatching young; which soon dive into the interior, and cover the root with blotches.

Sap conveyed by the wood alone.

Scab in potatoes.

The next part which belongs to the wood, and which I wish thoroughly to explain, is that which appertains to the balls. I have shown, that there are in plants two sorts, one which generally regulates the mechanism of plants, (the account of which I gave in my last letter on mechanical botany): the other the ball found in the wood of trees. When a bud, formed of the knot of the line of life, and passing through its covered way to the exterior, is by any accident severed from that line, the bud stops, and can proceed no farther, though the wood vessel joins to it. In time the albumen, which surrounds it, changes to wood, and the bud, though its interior never grows, yet continues

Ball found in the wood of trees.

to increase in wood, as long as the hardened parts about it will permit, adding one little row of wood each year, but this soon ceases. It is perfectly detached from the rest of the plant; and afterward pushed towards the exterior by the growing part of the plant, while the sap continues to circulate round it and within it. The constant pressure these balls receive makes them grow to an inconceivable hardness; and, when taken out, I have found them from twelve inches to a quarter of an inch in diameter, and so regularly formed, (see fig. 5) that had I not taken them myself from the tree, I should have been persuaded they were just turned in a lathe. Some are round, but with a single wood vessel attached to them; some formed like a spinning top. Here is an old tree, that has formerly been in a hedge-row, that has three balls about twelve inches in diameter. Carpenters, when they find them, use them as beads, cogs of wheels, or for any purpose that requires extreme hardness.

Balls in the wood.

The forming of hungry wood.

The next peculiarity of the wood I have never yet thoroughly explained is the exact reverse, in effect, of the ball; and the cause of both can be described by a drawing: it is what the French call hungry wood, it proceeds from some accident, a severe season, lightning, or injury the tree has received. Some wood is much more liable to it than others. It is a formation that is quickly finished, but that stops many of the sap vessels, so that the wood is soft and poor. I have often found a piece in the middle of beautiful perfect wood so diseased. Hungry branches are often seen shooting from the roots of trees. The rose, the viburnum, the barberry, and many shrubs as well as trees, are subject to this defect. Among trees the plane, the ash, and the lime in particular are most liable to it; it is to be known by its difference of appearance. To show it I shall draw a piece of solid good oak, and a piece so affected, having these specimens now in my possession. Fig. 3 is the healthy piece, fig. 4 the diseased oak: *c c* are the sap vessels, *d d* are the intermediate parts of both. When young, the wood is wider between the sap vessels, but not near so far apart as the diseased wood, which never appears to contract, as all perfect wood does. It is this effect, that has made many physiologists think, that the sap vessels dried up, when the wood

No wood without sap.

wood became old; but it only leaves the intermediate parts, which constantly contract. The sap vessels are rather enlarged than lessened by age; and grow vastly firmer by the strong support they thus gain by reduction.

Trifling difference in the albumen.

I shall now turn to the wood of annual or herbaceous plants: it is formed of two sorts, those which like the shrub have only a narrow piece of ligneous matter; and those the wood of which is formed in compartments, something like the manner of the leaf stalks, that is, round vessels of wood surrounded by albumen, which altogether appear infinitely larger than the common vessels so denominated, the spiral wire being enclosed within the wood. These vessels are set like spots in the circular pith; the number of sap vessels increases, if the plants last a long season, as well as the rows in the herbaceous vegetables, if fine weather prolong their existence: this I have before hinted at, in a former letter, Vol. XXVIII, p. 249. It would seem that this albumen, as well as that which surrounds the ligneous vessels in the leaf, never became wood; for let the vegetables be ever so old, it still retains the clear and unformed appearance it has at the first moment; and let it be where it will, it is always to be known. It is this we call nourishing vessels in the leaf. I think it should be called by the name of clear albumen, to distinguish it from that which afterward becomes wood. I have examined both; and, as far as the eye can judge, they appear perfectly the same; but in taste the clear albumen, which is that found in the seed, the leaf stalk, and herbaceous plants, is bitter, which the other is not, the other albumen is the new row of wood in trees and the foundation of the seed, with the first formation of the embryo. In all these the sap vessels afterward shoot. When I tried both by decomposition the residuum was the same; indeed the matter I can get for trial is so little, it is hardly a fair chemical process,

I should have continued to give an account of the ideas of Aubert du Thouars, could I have thoroughly understood the rest of his opinions; but he is not so intelligible as de Mirbel; and (if I may be allowed to say so) has rather too much given way to system and imagination. I should suppose it is necessary to pass from step to step in this study, perfectly distinguishing first the separate parts of a plant, and then the different sorts of vegetables, and keep to this; for if the seed is not exactly divided from the leaf, the stem from the root, and so on to the rest; and if the tree is not distinguished from the shrub, and the semiplant, and herbaceous plant, from the cryptogamia; it must inevitably create confusion, as they differ so essentially in form and manner of growing. It is also most desirable to give but one part at a time explaining this with a print, that may make it understood, or the most perspicuous writer, on so dark a subject, will want elucidation. But this gentleman, (though he began so well) has launched into some system, which he seems to hold out, I think, as some mathematical demonstration in the vegetable world. I regret that he has not divided it from his physiological labours, as both might have profited by this arrangement, I have however so much to apologize for myself, that I am the last person, that should criticise others.

Rest of du Thouars work.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

II.

On the Action of Elastic Fluids on dead Animal Flesh: by Mr. Hildebrandt.*

The putrescency of bodies must be much influenced by the surrounding gasses.

CONVINCED, that experiments and observations on the spontaneous analysis of organic substances must be very instructive, and tend to illustrate their nature, I have always endeavoured to impress on the hearers of my chemical lectures, how great must be the influence of the elastic fluids, that surround putrescent bodies, either in accelerating or retarding this natural process. This also has determined me, to investigate the subject with great care; and I venture to lay before the public the results, as constituting a series of experiments, that may tend to the advancement of the science.

Experiments on the subject.

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* Ann. de Chim. vol. LXXIII, p. 300. Translated from Gehlen's Journal, vol. VII, No. 2.

It will be proper to premise:

1st, That I always employed the same kind of flesh, namely beef, in order to be more certain, that the differences observed arose only from the action of the different elastic fluids: that the flesh used at one time was taken not only from the same animal, but from the same muscle; that it contained no fat, but muscular fibre alone; and that the pieces were of equal size, cut in parallelopipedons, and proportional to the vessels:

The substance employed.

2dly, That I took only the flesh of an animal that had been dead two hours:

3dly, That I used only the last portions of gas evolved, in order that it might not be mixed with any of the air in the receivers; and that I employed the gasses soon after they had been prepared. The atmospheric air I took from a very airy garden.

The gasses.

4thly, The vessels were placed in a room, into which the sun never entered. The windows also, facing the north, were very small, to avoid the action of light, which I purpose to examine on a future occasion. The temperature of this room is cool in summer; and in winter high enough to keep water from freezing. If, however, there were any reason to be apprehensive of frost in the night, I removed the vessels into my lecture-room, which joined it.

Place of the experiments.

Temperature.

I took three modes of enclosing the flesh in the gasses; and, to avoid repetition, I shall denote them by the following expressions: 1st, over water: 2d, over mercury: 3d, in an empty bottle.

The gas employed in three modes.

1. For the experiments over water, cylindrical jars were taken, containing from 92 to 98 cubic inches, Paris measure, and filled over a pneumatic trough. Pieces of meat 3½ inches long, 1 inch broad, and ¾ of an inch thick, were then introduced into them. These were suspended from two cross pieces of brass, having a point ½ an inch long to fix the meat on, and supported by a stem of the same metal, with two other cross pieces for a foot. When this stand, with the meat thus disposed, had been introduced through the water into the jar, a plate was passed into the trough, and the jar placed upon it, so that it might be removed from the trough, and set on a table. When the water rose in the

Manner of conducting the experiments over water:

the jar, which is the case when the temperature diminishes, or the gas is absorbed, care was taken to add water, in order to prevent the introduction of atmospheric air. In this way the contact of atmospheric air is avoided, but it is attended with the inconvenience of wetting the whole of the surface of the meat, and of the gas and meat being in contact with the water in the jar.

in a bottle stopped with a cork:

2. For the experiments in an empty vessel, that is to say, containing neither water nor mercury, bottles resembling those for wine were taken, but with wider mouths. These were filled with gas over the pneumatic trough, and let stand till they were well drained. The meat was then introduced, keeping the mouth of the bottle above the water, corked as quickly as possible, and the cork well luted with paper and glue, or the neck of the bottle placed in water. On turning up the bottle, the piece of meat falls to the bottom, to which it commonly adheres in consequence of its moisture. By this mode we avoid wetting the meat; and neither the meat nor the gas is in contact with so large a quantity of aqueous vapour: but it has this inconvenience, the meat has been in contact with atmospheric air, and a little is always introduced with it, when the cork is put into the bottle. This process cannot be employed with nitrous gas.

and over mercury.

3. For the experiments over mercury, small jars, of 3 or 4 cubic inches only, were filled with the gas to be subjected to experiment; and pieces of meat, an inch long, ½ an inch broad, and 2 lines thick, were then passed through the mercury with the fingers, and introduced into the jars. By this process the contact of atmospheric air is avoided; as well as of aqueous vapour, when the gasses have been procured over mercury. But these experiments I could execute only on a small scale, as I had not a sufficient quantity of mercury for any thing more.

A little water, or mercury, should always be left in the jars, in order that the air, when expanding, may not get out of them. In the experiments over mercury it affords the advantage of preventing the vessel from being overturned.

First series of experiments, over water, begun the 25th of March, and ended the 4th of April, 1808.

1st set of experiments over water.

Temperature.

The temperature of the air out of doors was always between 8° of Reaum. [50° F.] in the shade, as for instance on the 30th of March in the morning, and 5° R. [43·25° F.], as on the 25th of March at noon.

1. *Oxigen gas.*

Exp. 1. With oxigen gas from nitrate of potash.

The gas was obtained from nitrate of potash.

1st day. March the 25th. The meat is become evidently redder, and even of a finer red than in nitrous gas.

2d and 3d days. The red colour has diminished, but the meat has still a fresh appearance.

4th and 5th days. The same.

6th and 7th days. The redness has gradually diminished; and the meat is moister than that in nitrous or in hidrogen gas.

8th day. The meat grows damp; it begins to get livid; and little hemispherical and almost transparent drops appear standing separate from each other on its surface.

9th day. The drops become more numerous, and grow gradually opake and whitish; so that the meat looks as if covered with small pox.

10th and 11th days. Putrefaction makes evident progress, the meat grows flabby, the moisture increases, and the surface dissolves; yet the drops may still be distinguished at the surface of the liquid, that covers the meat entirely.

On the evening of the 11th day the meat was taken out, after the jar had been replaced in the trough. It emitted a putrid smell, somewhat alliaceous, having some resemblance to that of phosphuretted hidrogen gas.

The meat continued to putrefy in the open air, as if it had been constantly exposed to it.

The oxigen gas was not much diminished in bulk. It no longer set fire to a candle, but a candle still burned in it for a moment with a little brilliancy.

2. *Hidrogen gas.*

Exp. 2. With hidrogen gas

This was prepared by dissolving zinc in sulphuric acid diluted with water.

from zinc and sulphuric acid.

1st day. March the 25th. In the course of a few hours the meat had acquired a dirtier colour; and at length became of a blackish brown, like meat that has been smoked after having been salted without the addition of nitre.

From the 2d to the 11th day. No change in the external appearance of the meat; particularly it has grown neither flabby nor moist: its firmness appears even to have increased; and it seems more hard and dry. It has lost its redness more and more, and is become browner than meat exposed to carbonic acid.

On the evening of the 11th day the meat was taken out. It had no sign of putrescency, and not the least offensive smell: the most that could be said was, that it smelt slightly sour. On exposure to the air it did not putrefy, but became dry: a few small white specks of mouldiness, however, were perceivable on its surface.

On turning up the jar, and applying a lighted candle to it, the gas took fire.

3. *Carbonic acid gas.*

Exp. 3. With carbonic acid expelled from chalk by nitric acid.

This was prepared by dissolving chalk in nitric acid.

1st day. March the 25th. In the course of a few hours the meat had assumed a dirty colour, and afterward became brown; but it remained of a lighter colour than that in hidrogen gas.

From the 2d to the 11th day: At first it appeared to grow more livid and flabby, but after a few days no change was observed. The surface did not appear to get moist. The carbonic acid gas, used in the experiment, is absorbed by the water; and it even appears to be more readily absorbed than when pure, so that fresh gas must be added every day, to prevent the water from touching the meat.

On the evening of the 11th day the meat was taken out. It had no marks of putrescency; resembled meat that had been dressed; was flexible, without being moist or viscous; and had a slight acidulous smell, nearly like that of yeast turning sour. On exposure to the air it did not grow putrid, but dried, and its surface became covered with little white specks.

4. *Nitrous gas.*

Exp. 4. With nitrous gas from nitric acid and copper.

Procured from nitric acid by means of copper, and received over water.

1st. day. March the 25th. The meat became of a much finer red than in atmospheric air; and for the first few hours it could not be distinguished from that in oxigen gas.

2d and 3d days. No change.

4th, 5th, and 6th days. The fine red colour diminished a little, but it still remained very lively.

From the 7th to the 11th day. No change was observable, except that the meat appeared to grow a little moist; but it did not liquefy at all at the surface, and its firmness even seemed to increase.

On the evening of the 11th day the meat was taken out. It had a fine red colour, was firm, and had no smell, not even of nitrous gas. It lost its redness when exposed to the air, in the course of a few hours; became brown; and dried much quicker than the meat of the two preceding experiments. Its surface did not become covered with white specks.

The gas, exposed to the test of oxigen gas, produced a quantity of red vapour, diminished greatly in bulk, and appeared not to differ perceptibly from common nitrous gas.

Second series of experiments, from the 5th of April to the 10th of June.

Second series of experiments.

Temperature.

The temperature of the air out of doors was 2·5° [37·75° F.] on the morning of the 18th of April, and 23·3° [84·4° F.] on the 17th of May at noon.

That of the room was between 7° and 20° [47·75° and 77° F.].

Meat used.

The meat employed in the following experiments was of a paler colour, and appeared to have come from a younger animal.

5. *Oxigen from red oxide of mercury. Experiment over mercury.*

Exp. 5. Oxigen from red oxide of mercury, over mercury.

1st day. April the 5th. The meat became redder.

2d and 3 days. No perceptible change.

4th and 5th days. The colour grew paler.

From the 6th to the 8th day. The colour was destroyed, and the meat had the appearance of having been washed.

9th day. The drops appeared at the surface as in the first experiment.

18th day. The drops, became opake, appeared like the eruption of small pox. The meat preserved its firmness without liquefying, though the temperature was higher than in the former experiments.

From the 19th to the 21st day, May the 25th. Visible signs of putrescency were observed on the surface, the little drops ran into each other, and the surface became blackish.

An accident having overturned the jar, the gas escaped, and diffused such a strong stench throughout the house, that we were obliged to perfume it strongly, to get rid of the smell.

6. *Oxigen from nitrate of potash, in an empty bottle, stopped with a cork.*

Exp. 6. Oxigen from nitrate of potash in a corked bottle.

1st, 2d, and 3d day. The meat did not grow redder.

4th day. It grew pale.

5th to the 51st. No little drops were observed, but the meat gradully grew pale, became putrid, and liquefied at the surface. At length a considerable quantity of liquid, of a bad colour, was formed, and flowed down into the neck of the bottle.

The meat was covered with moisture. Its smell was not so strong as that of the preceding piece putrefied in oxigen gas, and of a different kind.

7. *Atmospheric air, over mercury.*

Exp. 7. Atmospheric air over mercury.

1st and 2d days. April the 5th and 6th. No remarkable change.

4th day. The meat is become very pale; paler than in the oxigen gas.

5th to the 51st. The little drops of liquid were not perceived. From the 8th day it became covered with moisture, and liquefied at the surface, but less than that in oxigen gas; and at the close it appeared less black than in pure oxigen gas, On taking it out of the receiver, its stench was not so powerful, and it appeared redder when cut.

8. *Pure hidrogen gas from the vapour of water passed over redhot iron. Over mercury.*

Exp. 8. Hidrogen from water by red-hot iron, over mercury.

1st day. April the 5th. The meat became of a crimson red.

From the 2d to the 51st. No change was observed, except that the meat became a little brown; but it did not acquire a livid hue. It is remarkable, that this meat remained reddish, and preserved an appearance of freshness, while pieces in oxigen gas, and in atmospheric air, grew pale. When taken out of the jar, it had no smell. The gas examined at the end of the experiment rendered lime-water turbid.

9. *Pure hidrogen gas in a corked bottle.*

Exp. 9. Hidrogen gas in a corked bottle.

1st to the 51st day. The meat scarcely grew brown at all, but preserved its colour, only appearing a little moist. When taken out the 51st day it had no bad smell, but smelt like smoked meat.

The gas, examined by the test of nitrous gas, gave no sensible diminution; it rendered lime-water a little turbid, and afterward burned vividly.

10. *Pure carbonic acid, from the calcination of chalk. Over mercury.*

Exp. 10. Carbonic acid, expelled from chalk by heat, over mercury.

1st day. The meat became crimsoned, as in hidrogen.

2d to the 11th. No sensible change, and the meat looks very fresh.

13th to the 22d day. It grew paler.

51st day. The meat was uniformly pale, looked as if it had been dressed, and appeared nearly of the same firmness.

It was neither moist nor viscous; and had not the least smell, or any other sign of putridity.

The gas was absorbed by lime, except a trifling residuum, that did not amount to 0·01.

Exp. 11. The former repeated in corked bottles.

11. If this experiment be repeated in corked bottles, and the meat be enclosed in one while the gas is hot, and in the other not till it has grown cold; it will be found, that the

meat in the cold gas will have kept well till the 16th day, but will have acquired an unpleasant smell; while that in the hot gas stinks on the 30th day, and is completely spoiled on the 60th.

12. *Nitrous gas, over mercury.*

Exp 12. Nitrous gas over mercury.

1st day. The meat became redder.

51st day. The meat has retained its fine colour, and is firm. The liquid, that has flowed from it, has assumed a fine red colour, and deposited a small portion of white matter, resemblimg fat, though the meat contained none.

67th day. June the 10th. The meat has retained its fine colour still, on which account I did not take it out, that I might see how long a time was necessary to effect its decomposition.

Third series.

Third series.

The temperature was the same as in the preceding series.

13. *Oxigen, over water, the jar contained only* 28·5 *cubic inches.*

Exp. 13. Oxigen gas over water.

1st day. The meat became of a fine red.

2d, 3d, and 4th. The meat retained its colour, and did not appear to putrefy.

6th day. Little transparent drops were observed; which increased in number and size on the 7th day, and grew turbid and red on the 8th.

9th day. The putrefaction was evident over all the surface, which began to liquefy. The gas diminished greatly in bulk. There is no doubt but the increase of temperature is the cause of the speedy putrefaction.

10th day. The gas, measured in the gasometer, had diminished 7 cubic inches. Being placed in contact with milk of lime, it diminished 0·5 cubic inches more. Supposing, that the 7 inches absorbed by water were carbonic acid, 13·5 inches of oxigen were expended, which must have formed 18·75 inches of carbonic acid gas[*].

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[*] The carbonic acid gas, as appears from the best recent experiments, must have been precisely the same in bulk as the oxigen gas expended. C.

The 15 inches remaining having been tested with nitrous gas, I found that they contained 5·4 of nitrogen, and 9·6 of oxigen. Thus the 28·5 inches of oxigen gas have been employed in

13·5 carbonic acid,
5·4 nitrogen,
9·6 oxigen.

14. *Atmospheric air.*

Exp 14. Atmospheric air, over water.

The meat putrefied, and was decomposed on the 48th day. The water rose considerably, and absorbed 21 cubic inches out of the 96, that the jar contained. The experiment having been deranged, I could not continue my observations.

15. *Pure hidrogen gas.*

Exp. 15. Hidrogen gas.

1st day. The meat became of a poppy colour.

4th day. No change, except that the meat appeared dried.

6th day. Some mouldiness observed, that increased on the 7th day.

From the 8th to the 41st day no farther change was observed, except that about the 20th the mouldiness had disappeared. The meat resembled beef salted without nitre and smoked. It had not the least bad smell.

The gas, tested with lime-water, did not render it turbid. It burned with force and energy.

From these experiments it appears how necessary it is, to repeat them separately, in order to obtain some certain results. It appears however, that we may draw from them the following conclusions.

General deductions.

1. That hidrogen preserves, and even increases the firmness of meat, by drying it. That oxigen on the contrary diminishes this firmness, rendering the meat flaccid and moist. It is remarkable, that hidrogen preserves this firmness even over water, when the gas is saturated with moisture.

2. That meat is changed and liquefied much more speedily in oxigen, when it contains nitrogen, as in atmos-

pheric air, and in the gas from nitrate of potash, than when the gas is pure.

3. That nitrous gas resists putrefaction most powerfully; next to which comes hidrogen, and then carbonic acid.

4. That meat does not change so soon in oxigen gas, as in atmospheric air; but, when putrefaction has commenced, it proceeds with more energy than in atmospheric air, and diffuses a much more offensive smell.

5. That the colour of meat gets browner in hidrogen, and lighter in oxigen and in nitrogen.

6. That neither hidrogen, nitrous gas, nor carbonic acid, appears to undergo any sensible alteration from the meat included in it.

7. That oxigen gas, whether pure, or mixed with nitrogen, is converted into carbonic acid.

8. That part of the oxigen gas retains its properties, as in other combustions.

9. That, during the putrefaction of the meat in oxigen gas, nitrogen is produced; which nitrogen must be evolved from the meat, or oxigen has been converted into nitrogen.

10. When meat begins to grow putrid in hidrogen, it appears, that carbonic acid is evolved; but this does not take place, as long as the meat keeps without spoiling.

11. That on meat in oxigen gas little drops of water are formed, which resemble the eruption of small pox.

Continuation of the inquiry.

A continuation of my inquiries will be instituted for the purpose of verifying the facts I have announced; and partilarly of ascertaining, whether the carbonic acid gas, found mixed with the hidrogen, existed in the meat; and of investigating the influence of light, and the luminous properties of stinking meat.

III.

Continuation of Mr. HILDEBRAND'S *Paper on the Action of Gasses on dead Animal Flesh.*

Meat left a long time in nitrous gas.

1. I Have said in my 12th experiment, that, finding the meat not altered after having remained 57 days in nitrous gas,

Temperature.

Results.

gas, I resolved to leave it there a longer time. Accordingly I did not take it out till the 25th of August, when it had remained in contact with the gas for 134 days. In the first months the temperature was between 7° and 20° [47·75° and 77° F.], and in the last months it was between 12° and 23° [59° and 83·75° F.] The temperature therefore was much higher, than was necessary to favour the decomposition of the meat, yet it retained a fine red and fresh colour. The liquor however, which had such a fine red colour, lost this in some measure. Having taken the meat out of the nitrous gas, to examine it more carefully, I observed, that, wherever it had touched the sides of the glass, it had become yellowish. (This I presume was owing to the contact of the glass diminishing the action of the gas.) In other respects, it was still of a fine red, had a good degree of firmness, and did not stink in the least; but it had a slight smell of nitric acid, with which a little of a peculiar smell was observed. Thus we see, that a longer time and a higher temperature produced changes, which did not take place in a shorter time and at a lower temperature.

Fibrin deposited.

The white sediment, mentioned in my first experiment, was found on examination to be coagulated fibrin. When agitated in water, it exhibited itself in the form of the little strings, that remain after the washing of the coagulum of blood. Boiling water did not dissolve, but hardened it.

The gas apparently unaltered.

The nitrous gas, that had been used for the experiment, when brought into contact with atmospheric air produced much red vapour; and the diminution of bulk was as great, as at the moment of its preparation.

Carbonic acid produced in hidrogen gas.

II. Having remarked in experiments 8 and 9, that the meat, which had remained 51 days in hidrogen gas at a temperature from 7° to 20° [47·75° to 77° F.], rendered lime-water turbid, I instituted the two following experiments.

16. *Hidrogen gas.*

Exp. 16. Hidrogen gas from zinc and sulphuric acid, over mercury.

I prepared this gas with zinc and dilute sulphuric acid, to obviate the objection, that the carbonic acid gas, contained in the hidrogen prepared by passing the vapour of water over a red-hot iron, might have originated from carburet of iron.

Temperature. I enclosed the meat over mercury. The thermometer out of doors, from the 23d of July to the 14th of September, was between 8·5° and 26° [51° and 90·5° F.]; that in the room between 11° and 20° [56·75° and 77° F.]. The shutters of the room were closed, except when the meat was examined.

Results. 1st day. July the 26th. In a few minutes the meat became of a brown and livid hue.

2d day. The colour became again a little red.

22d day. August the 17th. It retained a pretty red and fresh appearance, without any mark of putridity.

34th day. Every thing was in the same state, and appeared to remain so till the 54th day.

State of the meat the 54th day. Having taken the meat out on the 54th day, I found it as firm, as if it had been fresh; but it emitted an insupportable stench, differing however from that of meat putrefied in oxigen gas, or atmospheric air. Thus it appears, that organic matter can undergo a kind of alteration, and diffuse volatile principles occasioning noisome smells, without losing its cohesion, as in putrefaction properly so called.

Part of the gas converted into carbonic acid. The bulk of the gas in this experiment was diminished only according to the variation of the temperature. At 14° [63·5° F.] it was 4·75 cubic inches, French measure. When I passed it through lime-water, it rendered it very turbid; I therefore agitated it with milk of lime, and, having measured it afresh, it was reduced to 3 cubic inches, so that 1·75 cubic inch of carbonic acid had been formed.

17. *Hidrogen gas.*

Exp. 17. Hidrogen gas, over water. I placed a piece of meat in some of the same hidrogen gas; and the apparatus was kept with the preceding, and under precisely the same circumstances; except that the jar was over water, and contained 52 cubic inches.

Results. 1st day. July the 26th. The meat experienced the same change, as in the preceding experiment.

2d day. The colour appeared a little redder than in the experiment over mercury.

22d day. The meat looked red and fresh, and seemed drier than in the experiment over mercury. It appeared wrinkled.

The water has risen considerably in the jar, and rises every day.

54th day. The meat was of a fine red, but no longer wrinkled: it appeared drier, and resembled smoked meat. Taken out of the gas, it was as hard as smoked meat, but it diffused a horrible smell.

State of the meat on the 54th day.

As the temperature was 20° [77° F.] when the experiment began, and was now but 14° [63·5° F.], the quantity of 52 cub. in. should be reduced, according to Gay-Lussac, 0·14; and, according to Schmidt, 0·17: but there remained only 36 cub. in. of gas; consequently 15·83, or 15·86, were absorbed.

Diminution of the gas.

The gas, placed in contact with lime-water, and with milk of lime, diminished only a quarter of a cubic inch more; so that the total absorption was 16·08, or 16·11.

If the water and the milk of lime did not absorb as much gas in this experiment, as the milk of lime did after the experiment over mercury (for the ratio of 4·75 : 1·75 would give for the 52 cub. in., 19$\frac{3}{19}$), it is nevertheless evident, that in both the experiments there was a considerable formation of carbonic acid, amounting to more than a fourth of the bulk of the hidrogen employed.

A considerable portion of carbonic acid formed,

Though distilled water may contain carbonic acid, we cannot surely ascribe to it this great quantity of gas; particularly as it was continually rising in the vessel, so that it was absorbing the gas, and not giving it out. Besides, the experiment over mercury removes every doubt. The formation of carbonic acid therefore was owing to the decomposition of the meat: for hidrogen alone would remain for years over water or mercury, without being decomposed in any way. But though the gas, that remained after the separation of the carbonic acid, was hidrogen; it is equally certain, that part of the hidrogen had disappeared: for, had not this been the case, the bulk of the gas should not have been altered in the 17th experiment, and in the 16th it should have been considerably increased.

by the decomposition of the meat;

and part of the hidrogen lost,

As it is not probable, that this hidrogen combined with the meat in decomposition; since it appears, that meat gives out an hidrogenous vapour when it changes; it would follow, that the hidrogen, which disappeared, was combin-

apparently combined with or forming the carbonic acid.

ed in the mixture of carbonic acid gas, which I cannot explain.

IV.

On the Nonexistence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. In a Letter to ALEXANDER MARCET, *M. D., F. R. S. from* WILLIAM HYDE WOLLASTON, *M. D., Sec. R. S.*[*]

MY DEAR SIR,

Nonexistence of sugar in diabetic blood.

IN reply to your inquiry respecting my experiments upon the nonexistence of sugar in the serum of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

Attempt to detect it.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the diabetes (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

Farther experiments

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it; for, as I soon after[†] relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits, which are generally interesting to the Royal

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[*] Phil. Trans. for 1811, p 96.

[†] 1800.

Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments; requesting that you will in return state, more fully than you have hitherto done, the result of that farther step in the inquiry, which you took at my suggestion; and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

Dr. Rollo and Mr. Cruickshank had no opportunity of determining the point directly,

Although Dr. Rollo had been assisted in the chemical part of his inquiry by the well known talents of Mr. Cruickshank, it appears, that they "had not been so fortunate as "to obtain a sufficient quantity of serum for chemical ex-"periment*;" and were unable fully to satisfy themselves by the taste, or by other means which they could employ, concerning the existence or nonexistence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence.

but attempted it indirectly.

For the purpose of forming some judgment on this question, Mr. Cruickshank made trial of the quantities of oxalic acid, that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons.

Their method objectionable.

This method, it is evident, is liable to a twofold objection; first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed; and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result.

Method employed by the author.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient; for, though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

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* Rollo on Diabetes, p. 408.

The albumen in serum not completely coagulated by heat,

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exuded from it was still found to contain a portion of albumen dissolved in it; and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

unless a small portion of dilute acid was previously added.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation*. To six drams of serum I added half a dram of muriatic acid previously diluted with one dram and a half of water, and immersed the phial containing them in boiling water during four minutes. The coagulation was thus rendered complete. In the course of a few hours a dram or more of water exudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

Salt in the serum.

Effect of the presence of sugar.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness, that appears after evaporation: occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

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* I presumed, that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralising it.

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness, and disposition to deliquesce are of course more manifest than with smaller quantities*.

Sugar from diabetic urine had the same effects.

As I was aware, that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

Farther test of sugar.

As a farther test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts, that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop; and, if farther heat be applied, it becomes black in proportion to the quantity of sugar present.

Difficulties in experiments.

Such are the appearances, when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction, at the time when these experiments were conducted; and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances, which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself as to the phenomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

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* In Plate V, fig. 6, are represented the degrees of blackness of the drop occasioned by adding one, two, three, and four grains of sugar to six drachms of serum.

Trial with diabetic blood that had been dried.

The first which I examined was a portion of blood that had been taken from a person, whose urine had been analysed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be extracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

No sugar.

2d experiment.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

3d experiment.

In a third instance, I had some serum from the blood of a person, whose urine had been tasted, and found "*very sweet.*" (I had no opportunity of procuring any of this urine for analysis). After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic acid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

4th experiment.

I had also a fourth opportunity of examining serum of a person, whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat as sugar is: and its refractive power* was too great for that of sugar.

An equivocal appearance.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

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* The method by which this was tried has since that time been described in the Phil. Trans. for 1802. See Journal, vol. IV, p. 89.

5th experiment.

In the next experiment I added half a dram of the urine of the same person to six drams of the serum, and, with a due proportion of diluted muriatic acid, coagulated as before. Although the quantity of extract added did not exceed $\frac{1}{160}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

No sugar in serum.

The result of these trials was such, as to satisfy me, that the serum in this instance contained no perceptible quantity of sugar; or, at least, that the water separable from the coagulated serum did not contain one thirtieth part of that proportion, which I had found in the urine of the same person.

The sugar formed in the kidneys, or conveyed to them directly from the stomach.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood vessels. That some such channel does exist, Dr. Darwin* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it was conveyed to be the absorbent system, upon the supposition, that they might admit of a retrograde motion of their contents.

Dr. Darwin's attempt to prove this by nitre.

The author's by prussiate of potash.

Without adopting the theory of Dr. Darwin, it did appear to me, that the fact deserved to be ascertained by some test more decisive than nitre, and I conceived, that, if prussiate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found, that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

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* Account of the retograde motion of the absorbent vessels, by Charles Darwin.

Experiment.

After a few previous trials of the period, when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person, about thirty-four years of age, was induced to take a dose corresponding to three grains and a half of the dry salt, and to repeat it every hour to the third time. The urine, being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect: and as I thought, that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern, that any degree of blueness was occasioned by it.

The experiment repeated.

This experiment, having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

No prussiate found in the saliva,

Being desirous of ascertaining, whether the prussiate could be discovered in any other secretions, I have repeatedly examined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

or aqueous

I have also, during a severe cold, accompanied with profuse discharge from the nose.

fuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

Trial of the serum from a blister.

It was nearly in this state, that I left the inquiry at the period I have mentioned; and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum, that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance, by which substances may reach the bladder.

Retrograde action of the absorbents improbable.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Secretion generally effected by electricity.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope, that we might from this source derive some explanation of such phenomena. But though I have referred* secretion in general to the agency of the electric power with which the nerves appeared to be indued, and am thereby reconciled to the secretion of acid urine from blood that is known to be alkaline, which before that time seemed highly paradoxical; and although the transfer of the prussiate of potash, of sugar, or of other substances, may equally be effected by the same power as acting cause; still the channel through which they are conveyed remains to be discovered by direct experiment.

The channel still to be discovered.

I have, indeed, conjectured, that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu*; but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject.

I remain, dear sir, with great regard,
Yours very truly,

January 1, 1811. W. H. WOLLASTON.

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* Philosophical Magazine for June 1809.

V.

Dr. Marcet's *Reply to Dr.* Wollaston, *on the same Subject.*

Russell Square, January 8, 1811.

MY DEAR SIR,

The hypothesis of the presence of sugar in diabetic blood specious.

I Am much gratified to find, that you have at last been induced to communicate to the Royal Society your curious inquiry respecting the state of the blood in diabetes. I was anxious, that the specious hypothesis of the presence of sugar in diabetic blood, which had been sanctioned by the authority of Dr. Rollo and Mr. Cruickshank, and which I had myself urged in support of their theory, fourteen years ago, in an inaugural publication, should no longer obtain an undue weight among physiological inquirers.

Attempt to ascertain whether prussiate of potash, taken internally, be present in any other secretion beside urine.

With regard to the experiments which I tried at your request some years ago, with a view to ascertain whether prussiate of potash taken into the stomach, and found to exist in the urine, could also be detected in other secretions, I find, on referring to my memorandums, the following particulars, which I shall transcribe verbatim.

" August 19, 1807. Having heard from Dr. Wollaston, that prussiate of potash could be taken into the stomach with perfect safety, and that its presence could afterward be discovered in the urine, but not in the serum ; and being invited by him to follow up this inquiry, with a view to connect it with the theory of diabetes, I tried the following experiments.

Exp. 1. Prussiate of potash may be taken in considerable doses.

Exp. 1. " After having satisfied myself, by trials made by some medical gentlemen upon themselves, that considerable doses of prussiate of potash might be taken without the least inconvenience, I gave to a young woman, labouring under diabetes mellitus, five grains of prussiate of potash dissolved in water, and this was repeated every hour, till she had taken thirteen or fourteen such doses. After the fifth dose, her urine, by the addition of a drop or two of a solution of sulphate of iron, turned blue instantly. At this period of the experiment, a blister was applied to her stomach, and after a few hours, while still taking the prussiate of potash, and while the urine strongly indicated its presence, the blister was cut, and the serum collected. This serous fluid being, in the same manner as the urine, subjected to the action of a solution of sulphate of iron, did not suffer any change of colour in the least indicative of the presence of prussic acid. Yet the urine still remained capable of imparting a blue colour to solution of iron, fifteen hours after taking the last dose of the prussiate of potash.

None in the serum from a blister.

Exp. 2. Sulphate of iron taken.

Exp. 2. "The same person being soon afterward put upon a course of ferruginous medicines, and having taken considerable quantities of sulphate of iron, an idea naturally occurred to me, that the phenomenon might perhaps be reversed; but upon adding prussiate of potash to the urine, no vestige of iron could be discovered, and the same attempt was repeated several times with the same negative result.

No iron in the urine.

Exp. 3.

Exp. 3. "Dec. 2, 1807. The fluid obtained by means of a blister (as in Experiment 1.) being not immediately derived from the circulation, since it may be considered as the product of a secretion, I was desirous of repeating Dr. Wollaston's experiment on the serum itself, under circumstances of impregnation similar to those, in which the serum of the blister was examined.

"For this purpose, a young woman, after taking, in divided doses, about a dram of prussiate of potash in the course of twelve hours, lost some blood by cupping, an operation which had been ordered for a local complaint under which she laboured. The serum having been allowed to separate, and a little nitric acid having been added to it, not the least vestige of prussic acid appeared on applying the test of sulphate of iron, although the urine, made during the six hours which preceded and followed the cupping, was strongly impregnated with that acid, and struck a vivid blue upon adding the smallest quantity of iron."

Prussiate of potash detected in the urine, but not in the serum of the blood.

Prussiate of potash not conveyed to

I have only to observe, in addition to these particulars, that the susceptibility, by which prussiate of potash is transmitted the bladder in all persons with the same facility.

mitted to the bladder, seems to vary in different individuals; for in five trials, made at Guy's Hospital in Nov. 1805, I failed of discovering any vestige of this salt in the urine of persons, who had taken it in quantities sufficient to produce its appearance in others. Three of these individuals, I should observe, were at the time under mercurial treatment; and an idea occurred to me, that, mercury having a great affinity for prussic acid, the presence of this metal in the system might prevent the effect in question. But, as in the two other failures no mercury was present, I cannot lay any stress upon this conjecture. It may be proper to mention, that, in the frequent trials which I have made with the prussiate of potash, no symptom or inconvenience whatever has ever occurred, which could be ascribed to this salt.

No inconvenience from taking it.

I remain ever,
My dear sir, with great esteem,
Yours sincerely,
ALEX. MARCET.

The channel of conveyance may be the arteries and lymphatics, to the exclusion of the veins.

P. S. While revising the proof of this sheet, it has been observed to me by some friends, and in particular by Dr. Henry of Manchester, and Dr. R. Pearson of London, that, in order to show distinctly that certain substances find their way to the bladder, without passing through the general circulation, it would be necessary to examine the arterial, as well as the venous blood, since it is not impossible, that the whole of the sugar in diabetes, or the prussiate of potash in the experiments above related, may be conveyed to the urinary organs by the arteries, without entering the venous system. According to this hypothesis, it may be conceived, that the same substances, when conveyed by the arteries to distant parts of the body, may return by the absorbent system, and might in this case be discovered in the thoracic duct. This view of the subject may deserve farther investigation; and I hope, that this curious question will soon be decided by appropriate experiments.

VI.

On the Algorithm of Imaginary Quantities. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Defective algorithm of imaginary quantities.

As neither of the papers which you did me the honour to insert in your Journal, "On the Defective Algorithm of Imaginary Quantities*," has been noticed by any of your mathematical correspondents, I now propose to fulfil my promise, viz. "That if no answer appeared within three months, I would then, through the medium of your Journal, give my own explanation of the difficulty." I cannot, however, but regret, that, amongst the many able advocates for the introduction of these quantities into mathematical investigations, no one of them should have stepped forward to explain an anomaly attending such introduction, which seems to militate so much against the boasted certainty of results, deduced from mathematical investigations.

Results from imaginary quantities uncertain.

The defect pointed out in my former letters is, I believe, a new case, which has not been noticed by any of the writers on the subject; and, unless it can be satisfactorily answered, it is impossible to deny, that uncertainty and doubt must necessarily attend all results obtained through the medium of imaginary quantities. I am sorry, through the backwardness of your other correspondents, it has fallen to me to enter upon this subject; I will, however, do my best to explain the anomaly in question; at the same time I cannot but again repeat my regret, that it has not been taken up by some one more competent to the task.

Origin of imaginary expressions.

First then, let us consider the origin of these imaginary expressions, which may be traced to the very first principles of algebra. We are taught in multiplication, that $(+a)^2$, and $(-a)^2$, are both to be represented by a^2; and consequently, when the converse of this operation arises, or when we are required to extract the square root of a^2, it is either $+ a$, or $- a$; but if it be required to extract the square root of $- a^2$, we must necessarily be stopped in our progress, for there being no previous convention, that $- a^2$ shall represent the square of any quantity; it follows, that, when such a case arises, we are not able to assign to it any particular root, and we are thus presented with the first and the most simple form of imaginary quantities. These expressions, however, though they have no definite value, yet, considered as mathematical symbols, ought to be subject to certain rules, as well as other symbols, which are the representatives of real quantities; the rules, however, which are laid down for operating in the latter case, frequently require certain modifications before they can be applied to the former, and most of the difficulties, which have occurred with regard to the algorithm of imaginaries, have arisen in making rules general, which were first intended to answer only in particular cases.

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* Vol. XXIX, p. 254, and XXX, p. 209.

Difficulties from applying generally what was intended for particular cases.

The ambiguity does not exist in certain cases.

We have seen above, that $\sqrt{a^2} = \pm a$; that is to say, it has an ambiguous root, which may be taken either at $+ a$, or $- a$; but this ambiguity has not place, if we know how the quantity a^2 was generated, and have occasion afterwards to retrace the steps of our operation: we cannot, for instance, say, that $\sqrt{-a \times -a} = \sqrt{a^2} = \pm a$; or that $\sqrt{+a \times +a} = \sqrt{a^2} = \pm a$ for the square root of a in both these cases is determined; that is, when considered with regard to its generation, it has but one root; whereas its origin not been known, we must have prefixed the ambiguous sign to the root a, and for this obvious reason, that we know not, when a^2 is unconditionally assumed, whether it be the representative of $(+ a)^2$, or of $(- a)^2$; these being both expressed by the same symbol a^2. In fact, there is no ambiguity in the extraction of the roots of quantities, except in those cases in which we are unacquainted with their generation; and in these it must necessarily arise, because we have agreed to represent the powers of different quantities by the same symbol.

Caused by expressing different quantities by one symbol.

This illus.

There are, for instance, three different quantities, which, being trated by an example.

being cubed, give unity for the result; we say, therefore, that 1 has three roots; and, if we are simply requested to extract the cube root of 1, we may make it either 1 or $-\frac{1}{2}+\frac{1}{2}\sqrt{-3}$, or $-\frac{1}{2}-\frac{1}{2}\sqrt{-3}$; for the cube of each of these quantities is represented by 1. But if we are asked, what is the cube root of $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})^3$, we must not say, that $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})^3=1$, and therefore its root is also equal to 1; for, as in this case we know, that 1 is the representative of the cube of a particular quantity, its cube root must necessarily be that quantity, and no other.

No ambiguity when we know how the quantity was generated, unless it was the result of unequal factors.

Hence it appears, that there is no ambiguity in extracting the roots of quantities, which are known to be generated from the constant multiplication of a given quantity by itself, whether this quantity be real or imaginary. But if, by the multiplication of two unequal factors, we arrive at any result, and have afterwards to extract the root of that quantity, there is then nothing in the nature of the case to limit the root. If, for example, we find, that the product $(-\frac{1}{2}+\frac{1}{2}\sqrt{-3})\times(-\frac{1}{2}-\frac{1}{2}\sqrt{-3})=1$; and we have afterwards to take the cube root of this product; there is nothing to indicate, that we ought to take one root in preference to another: whence it follows, that a quantity generated from the product of unequal factors has all the generality of a quantity unconditionally assumed; whereas that which is generated from the product of equal factors has not that generality, as it admits only of one root; while the other quantity, made up of unequal factors, has as many roots as there are units in the index of the power, the root of which is to be extracted.

Application to the point in question.

Let us now see how far what has been said above will serve to explain the difficulty stated in my former letters, or rather in my last letter, to which I intend more particularly to direct the following remarks:

The quantity which I proposed to square was the following,

$$\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}.$$

Now here the quantities under the cubic radicals are the two imaginary roots of the equation $x^3=1$; which, for the sake of simplicity, we will represent by a and b, the third root being 1. Then from the known theory of equations we have $1 + a + b = 0$, or $a + b = -1$;

also $1 \times a \times b = ab = 1$;

Answer to the question on imaginary quantities.

Now we have also $a^3 = 1$; $b^3 = 1$; but there is this difference with regard to these products represented by 1; viz. that the first has no limitation as to its roots, being the product of different factors, and is therefore as general as 1 unconditionally assumed; whereas the two latter have only particular roots, the first being a, and the second b, and they can have no other roots; if, therefore, in the operation of squaring, we have to take the roots of these quantities, we must pay particular attention to this circumstance; the want of which, in the numerical example in my former letter, is what gave rise to the incongruous result there deduced. Let us now square this literal expression by the common method, only observing, with regard to the products and powers, the rules above laid down:

$$\begin{array}{l} \sqrt[3]{a} + \sqrt[3]{b} \\ \sqrt[3]{a} + \sqrt[3]{b} \\ \hline \sqrt[3]{a^2} + \sqrt[3]{ab} \\ \qquad + \sqrt[3]{ab} + \sqrt[3]{b^2} \\ \\ \sqrt[3]{a^2} + 2\sqrt[3]{ab} + \sqrt[3]{b^2} \\ \sqrt[3]{a^2} + 2 + \sqrt[3]{b^2} \end{array}$$

And that this is the true result, may be shown as follows. The proposed formula

$$\sqrt[3]{-\tfrac{1}{2} + \tfrac{1}{2}\sqrt{-3}} + \sqrt[3]{-\tfrac{1}{2} - \tfrac{1}{2}\sqrt{-3}}, \text{ or}$$

$$\sqrt[3]{a} + \sqrt[3]{b}$$

is the real root of the equation

$$x^3 - 3x = -1,$$

as deduced from Cardan's rule; and consequently the cube of that formula, *minus* three times itself, ought to be equal to -1. Let us therefore continue the operation, and see how far

Answer to the question on imaginary quantities.

far the result will agree with the required conditions: for this purpose we will repeat again the preceding square, viz.

$$\sqrt[3]{a^2} + 2 + \sqrt[3]{b^2} = (\sqrt[3]{a} + \sqrt[3]{b})^2$$
$$\sqrt[3]{a} + \sqrt[3]{b}$$

$$\sqrt[3]{a^3} + \sqrt[3]{a} + \sqrt[3]{ab^2}$$
$$+ \sqrt[3]{a^2b} + 2\sqrt[3]{b} + \sqrt[3]{b^3}$$
$$a + 3\sqrt[3]{a} + 3\sqrt[3]{b} + b = (\sqrt[3]{a} + \sqrt[3]{b})^3$$

This result is evidently deduced according to the preceding rules, in which it is shown, that $\sqrt[3]{a^3} = a$, $\sqrt[3]{b^3} = b$, $\sqrt[3]{ab} = \sqrt[3]{1} = 1$, and consequently $\sqrt[3]{a^2b} = \sqrt[3]{aba} = \sqrt[3]{a}$ and $\sqrt[3]{ab^2} = \sqrt[3]{ab\cdot b} = \sqrt[3]{b}$

We have seen also, that, a and b being the imaginary roots of the equation $x^3 = 1$, $a + b = -1$; the above expression may therefore be written

$$x^3 = (\sqrt[3]{a} + \sqrt[3]{b}) = 3(\sqrt[3]{a} + \sqrt[3]{b}) - 1$$

also $-3x = 3(\sqrt[3]{a} + \sqrt[3]{b}) = 3(\sqrt[3]{a} + \sqrt[3]{b})$

whence $x^3 - 3x = -1$

as it ought to be; which shows, that our operations have been accurately performed.

It only remains, therefore, now to explain in what respect our numerical operation differs from that which has just been performed upon the literal symbols a and b. Now, upon a comparison of the two, we shall find them to be precisely the same, except, that in the former we have not retained the square form under the radicals; by which we lose sight of one of the particular circumstances, which ought to be kept in view throughout the operation. It happens in the example proposed, that the imaginary quantities under the radicals are powers of each other; as is the case with the roots of every binomial equation $x^n = 1$; that is, in our particular examples, $a^2 = b$ and $b^2 = a$: but this equality has not place when the quantities enter under a radical, and the

Answer to the question on imaginary quantities.

the reason of this restriction is obvious; because though $a^2 = b$; and consequently $a^3 = ab$, which are both represented by unity, or 1, yet the cube root of the former is a, and of the latter 1: as is evident from the preceding part of this paper, the first (1,) being produced from the cubing of a certain quantity a, and therefore having its cube root $= a$; and the other, being the product of two unequal factors, possesses all the generality of 1 unconditionally assumed.

Hence it appears, that there may be quantities equal to each other, which, when placed under the same radicals, lose their equality: the one of them being restricted to give a particular root, and the other a different root; and therefore, in such cases, although the quantities and the radicals are precisely the same, yet all equality between the two ceases, and they must be considered as totally dissimilar quantities.

This is exactly the case in the examples proposed, as may be made evident as follows:

We have seen, that $a^2 = b$, and $b^2 = a$; and if, in consequence of this equality, we make our first result, obtained from squaring, viz.

$$\sqrt[3]{a^2} + 2 + \sqrt[3]{b^2} =$$
$$\sqrt[3]{b} + 2 + \sqrt[3]{a}$$

as we did in the numerical example, we should find, that in continuing the operation, we should not arrive at the same result as in the former case; that is, by multiplying again, in order to cube the expression, we should have

$$\begin{array}{l}
\sqrt[3]{b} + 2 + \sqrt[3]{a} \\
\sqrt[3]{a} + \sqrt[3]{b} \\
\hline
\sqrt[3]{ab} + 2\sqrt[3]{a} + \sqrt[3]{a^2} \\
\qquad + \sqrt[3]{b^2} + 2\sqrt[3]{b} + \sqrt[3]{ab} \\
\hline
\end{array}$$
$$1 + 3\sqrt[3]{a} + 3\sqrt[3]{b} + 1 =$$
$$3\,(\sqrt[3]{a} + \sqrt[3]{b}) + 2$$

which is different from the former result.

The difficulty therefore stated in both my former letters is thus explained, namely, that we have considered quantities as equal, because they are represented by the same symbols; whereas, in consequence of thus entering under radicals, and being restricted with regard to their roots, all equality between them ceases.

Method of guarding against such anomalies.

With regard to the proper criterion, by which such anomalies may be guarded against in other cases, there appears to be none more general than the following, viz.—that, in the involution of quantities under radicals, the operation should not be worked out at length, but indicated by the sign of the particular power, to which it is to be raised.

I have thus endeavoured to fulfil the promise which I made in my former letter; but how far what I have said may be considered as satisfactory must be left to others to decide. I shall only observe, that if any of your correspondents should perceive any defect in the reasoning employed in the preceding pages, I shall be extremely happy to see it corrected in a subsequeat number of this Journal; if not, I may probably at some future time trouble you with a few other remarks on this subject.

Yours,

MATHEMATICUS.

VII.

Description of a Compensation Pendulum for a Clock: by Mr. ADAM REID, *of Green's End, Woolwich*[*].

SIR,

Half-second compensation pendulum.

YOU will have the goodness to lay before the Society of Arts &c. a half-second compensating pendulum of my invention; which is so simple in its construction, that it will be fully understood by viewing either the pendulum, or the drawing which accompanies it. I believe it to be new, and wishing it to be useful to the world, I have presumed to send it to the Society. I am, sir,

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[*] Trans. of the Soc of Arts, vol XXVIII, p. 230. Fifteen guineas were voted to Mr. Reid for this invention.

Your humble servant,
ADAM REID.

Reference to the Drawing of Mr. Reid's Compensation Pendulum, Pl. VI.

The pendulum described.

Fig. 1 and 2, Pl. VI, A B represent the steel rod extending through the whole. C the bob, supported upon the compensating cylinder of zinc D, which surrounds the rod A B, and rests upon the nut E of a screw tapped upon the end of the steel rod, to bring it to exact time; as this expands downwards by heat, the zinc expands upwards the same quantity; so that the bob always remains at the same distance from the point of suspension. Fig. 2, is a section to explain more clearly the thickness of the zinc tube D, and the form of the steel rod at *a*, where it passes through the bob, which is of the shape shown at L, that the rod or the bob may not turn round, when the nut E is turned to adjust it to time.

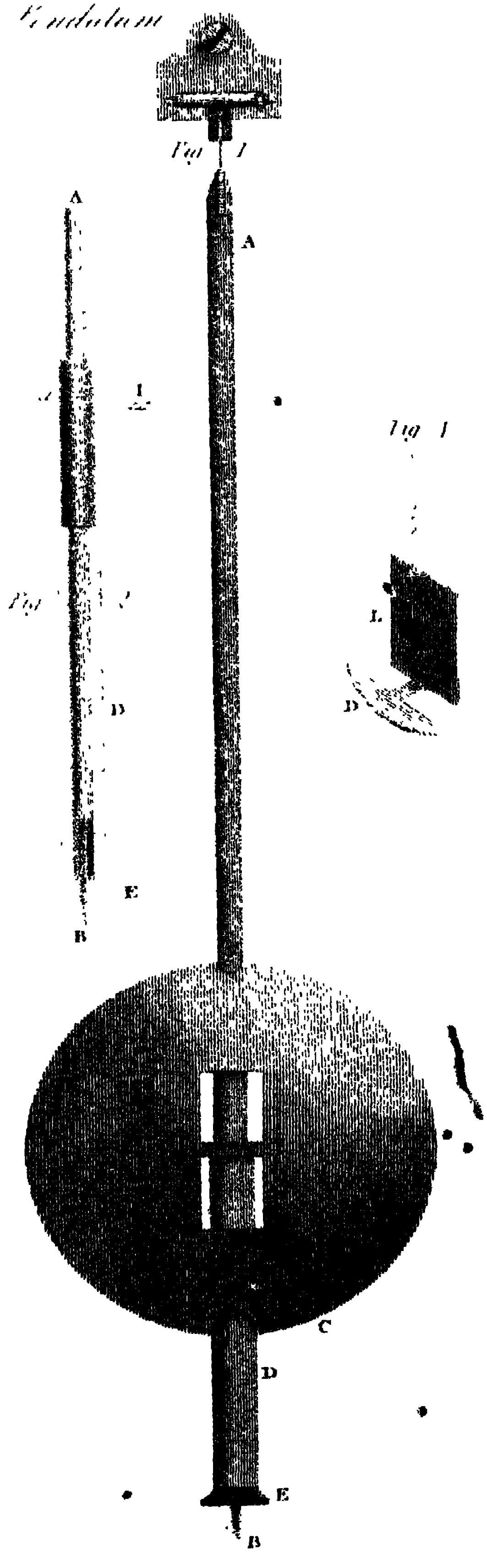
The following Method is to be employed to make the Compensating Pendnlum of Steel and Zinc.

Method of making it.

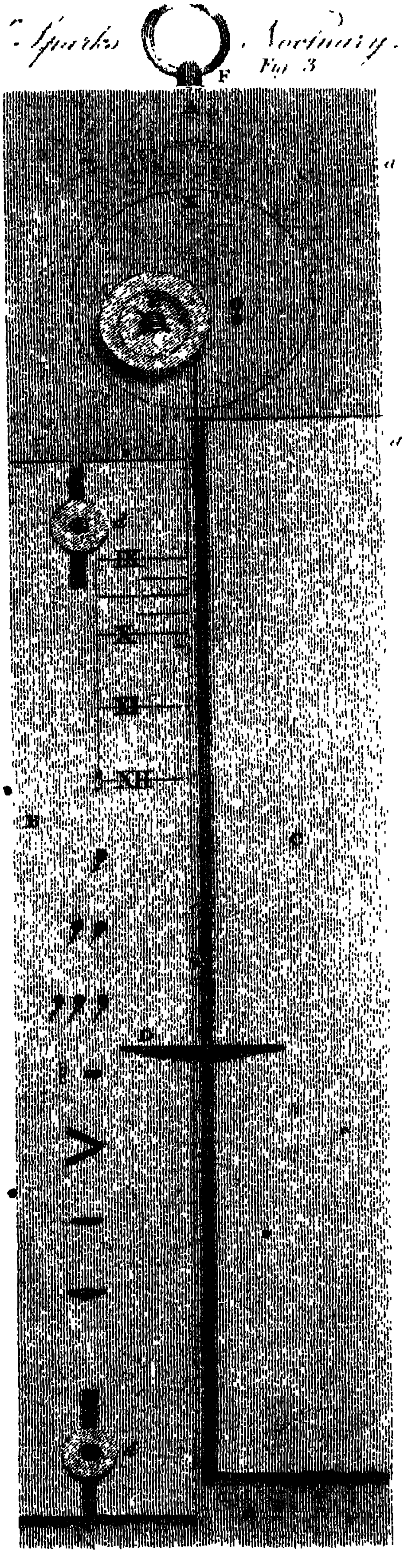
Procure a rod of forged blister-steel 52·7 inches long, ·27 diameter, heat it to a white heat, which will open the pores of the steel, and give it the smallest expansive power, that steel of this texture is possessed of. When cold straighten it with a mallet of wood on a wooden block, that no part may be condensed partially; which would be the case, if a hammer and anvil were used. Then cast a solid rod of zinc 12·5 inches long, ·68 diameter, with the lowest heat, that will fuse it; and pour it into a metal mould. This will give it the greatest density, consequently the greatest expansion, that zinc is possessed of. Then bore a hole through the centre of it longitudinally, that it may move freely on the steel rod, which has a nut and screw at the bottom end to regulate the clock to time; the bob, as shown in the engraving, rests on the upper end of the cylinder of zinc, and will continue in the same place, whatever expansion or contraction takes place, if the adjustment be correct.

If

Reid's Compensation Pendulum



Mr. Sparks Noctuary.



Platina & steel still better.

If platina was used instead of steel, and steel instead of zinc, a pendulum might be made equally good, and more compact; but not at so small an expense.

The dimensions above for a second pendulum.

The above dimensions are to be understood in the finished state of their diameters and lengths, proper for a second pendulum.

Answered on trial.

I have constructed a pendulum on this principle, which has been in use some months, and I have the satisfaction to find it has answered my expectations; the temperature of the room was from 58 to 34 degrees, and no variation of the clock, when compared with my other clock, which, from many years trial, I know to be a good one, in a room where the thermometer does not vary more than four degrees.

Inconvenience of a compound metal,

The difficulty, or rather impossibility of making a good pendulum, where a compound metal as brass is employed, arises from the circumstance, that neither brass nor any compound metal can be made uniform, not even for one foot in length; and then if drawn into wire the parts acquire a longitudinal grain, which adds to the variation of the expansive powers. To avoid this, zinc has been substituted, with no more certainty of success; for if a pendulum is made in the summer, and the steel pins fill the holes well, and it is exposed to a severe frost, and put to the clock for only a few years, the invisible fissures will become visible in the performance of the best clock, and a visible separation render the pendulum useless; as I have witnessed.

and of zinc.

I am willing to furnish the Society with any farther information in my power upon the subject.

Woolwich, April 27, 1809. ADAM REID.

VIII.

Method of ascertaining the Hour in the Night, by an Apparatus connected with a common Watch; by Mr. G. SPARK, *of Elgin, Murrayshire, Scotland**.

SIR,

Invention for knowing the

BY Mr. John Newton, watchmaker, I have forwarded an invention for knowing the hour in the dark, by feeling.

I think

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* Trans. of the Soc. of Arts, vol. XXVIII, p. 234. The silver medal was voted to Mr. Spark for this invention.

hour in the dark.

I think it preferable to a repeater, on account of its simplicity and cheapness. It is not liable to be out of order, and it does not require the exertion necessary for pushing the pendant of a repeater, or disturb any person near it.

For these reasons I diffidently wish to have the honour of laying this invention, which I call a Noctuary, before the Society; and to be favoured with their decision on its merits.

I am, sir,
Your respectful and obedient servant,
Elgin, March 7, 1810. GEORGE SPARK.

Reference to the Engraving of Mr. Spark's Noctuary, Pl. VI, *Fig.* 3, *and* 4.

The apparatus described.

A A, fig. 3, is a mahogany board, upon which two others, B and C, are fixed, so as to form a groove beneath and underneath them, in which the index D, shown separately at fig. 4, descends.—On the opposite side of the board a flap or door of mahogany is fixed by two hinges, *a a*, and a clasp; between this and the board A A a cavity is formed to contain the watch, as shown by the dotted circle X; the dial appearing through a circle in the door, a hole is made through the board A, opposite the fusee square, to receive a key, upon which the small pulley E is fixed, and from which the index D is suspended by a fine thread in the groove above-mentioned. It is plain, that, as the fusee-square and pulley E revolve, the index descends, and points out the hour by coming opposite the several marks, commencing at nine and ending at seven, which are fixed upon the board B; the marks, from twelve to seven, are made by pins projecting from the surface, so as to be readily distinguished by the finger; the index, represented in perspective at L, fig. 4, is made very light, that it may not influence the motion of the watch. The watch must be wound up before it is placed in the frame, and the thread wound up on the pulley E, so much as to suspend the index nearly the height of the hour when it is set; the key is then pushed on the fusee-square, and if the index does not point exactly at the right hour, the scale B can be slided up or down to adjust it; the screws *d d*, which hold it, being fitted in grooves for this purpose; after this setting, the index will point out any succeeding hour, descending a division at each. F is the ring by which the instrument is suspended; and G is a hole, in which the key on the pulley E is placed, when the watch is removed, and the instrument out of use.

IX.

On the Management of the Onion. *By* THOMAS ANDREW KNIGHT, *Esq. F. R. S.*, *&c.**

Culture of useful plants a prime object.

THE first object of the Horticultural Society being to point out improvements in the culture of those plants, which are extensively useful to the public, I send a few remarks on the management of one of these, the onion; which both constitutes one of the humble luxuries of the poor, and finds its way in various forms to the tables of the affluent and luxurious.

Growth of bulbs.

Every bulbous rooted plant, and indeed every plant which produces leaves, and lives longer than one year, generates, in one season, the sap, or vegetable blood, which composes the leaves and roots of the succeeding spring; and when the sap has accumulated during one or more seasons, it is ultimately expended in the production of blossoms and seeds. This reserved sap is deposited in, and composes in a great measure, the bulb; and moreover the quantity accumulated, as well as the period required for its accumulation, varies greatly in the same species of plant, under more or less favourable circumstances. Thus, the onion in the south of Europe acquires a much larger size during the long and warm summers of Spain and Portugal, in a single season, than in the colder climate of England; but under the following mode of culture, which I have long practised, two summers in England produce nearly the effect of one in Spain or Portugal, and the onion assumes nearly the form and size of those thence imported.

The onion.

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* Trans. of the Horticultural Society, vol. I, p. 157.

method of obtaining onions equal to the Spanish,

Seeds of the Spanish or Portugal onion are sown at the usual period in the spring, very thickly, and in poor soil; generally under the shade of a fruit tree: and in such situations the bulbs in the autumn are rarely found much to exceed the size of a large pea. These are then taken from the ground, and preserved till the succeeding spring, when they are planted at equal distances from each other, and they afford plants, which differ from those raised immediately from seed only in possessing much greater strength and vigour, owing to the quantity of previously generated sap being much greater in the bulb than in the seed. The bulbs, thus raised, often exceed considerably five inches in diameter, and being more mature, they are with more certainty preserved, in a state of perfect soundness, through the winter, than those raised from seed in a single season, The same effects are, in some measure, produced by sowing the seeds in August, as is often done; but the crops often perish during the winter, and the ground becomes compressed and soddened (to use an antiquated term) by the winter rains; and I have in consequence always found, that any given weight of this plant may be obtained, with less expense to the grower, by the mode of culture I recommend, than by any other which I have seen practised.

and excellent for keeping.

X.

Hints relative to the Culture of the Early Purple Brocoli, as practised in the Garden of Daniel Beale, Esq. at Edmonton. By Mr. JOHN MAHER, *F. H. S**.

Brocoli much improved.

FEW vegetables have been more improved of late years than brocoli, so that it now almost equals in flavour and magnitude the delicate cauliflower, over which it has the decided advantage of being more hardy, and may, by a little management, be procured through the whole winter.

Whiteness a perfection in plants of this family.

Several varieties, differing in colour from white to deep purple, are sold by our nurserymen; and as all plants of this natural family become less alkalescent and more palatable in proportion as they approach to a pale or white colour, such varieties will undoubtedly be preferable to purple ones, if they turn out equally hardy: nor are we to despair of raising them, by patience and perseverance in selecting the largest and whitest specimens of the common brocoli for seed.

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* Trans. of the Horticultural Society, Vol. I, p. 116.

Difficulties in the pursuit.

All attempts of this kind however demand both a long time, and no trifling expense; nor can they be easily prosecuted, except in the insulated grounds of those gentlemen, whose liberality, like that of my master, rivals their extensive possessions: for, out of a great number of plants set apart for seed, perhaps not even one may answer our wishes; and if a* brisk gale of wind, or wandering bees, bring the pollen of any other variety to their flowers, the progeny, in ninety-nine instances out of a hundred, will be deteriorated instead of improved, and in no case prove the identical variety sown.

Excellent brocoli of a large size.

The brocoli, of which I am now emboldened to offer some account to the Horticultural Society, is reported to have been introduced from the Cape of Good Hope, by the Hon. Marmaduke Dawnay, and first cultivated in Surry, where it is called the early Cape brocoli. Packets of seed, first sent here from Italy, which appear to me to have produced the same variety, have also been sold for two seasons by Mr. Grange, fruiterer, in Covent Garden and Piccadilly: it may therefore easily be obtained, and our principal care now must be to preserve it in its present magnitude and excellence.

Time of sowing the seeds.

My method of treating it is as follows. Three crops are sown annually: the first between the 12th and 18th of April: a second between the 18th and 24th of May: the third between the 19th and 25th of August: these successive crops supply the family from September till the end of May.

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* The result of an action for damages brought in Westminster Hall more than a century ago, against an innocent but unfortunate gardener, for selling cauliflower seeds, which only produced long-leaved cabbages, has been stamped with immortality by the pen of Linneus, in his celebrated treatise on the sexes of plants, the Sponsalia Plantarum, and confirms this remark of the author's very forcibly. Secr.

Sowing, The seeds are scattered exceedingly thin, in a border of very rich light earth. Not a weed is suffered to appear; and when the young plants have from 8 to 10 leaves, which is in about a month, they are finally planted out at the distance of two feet every way, in a piece of sandy loam, which has been well prepared for the purpose by digging, and enriching it with a large portion of very rotten dung, frequently turned over to pick out every sort of grub, or insect deposited in it. The ground is kept constantly clean by hoeing, whenever a seed leaf of any weed springs up; and the loose surface is drawn together into a heap, round the stem of each plant.

and planting out.

Second crop for winter use. The second crop is treated exactly as the first; but the weaker plants left in the seed bed are permitted to remain 8 or 10 days longer, to gain more strength. They are then transplanted into pots of the size called sixteens, filled with very rich compost, placing them close to each other in the shade, and duly watering the plants, till they begin to grow freely. After this, the pots are plunged in the open ground at two feet distance from each other every way, and about 3 inches under the general level, leaving a hollow or basin round each plant, to retain any water given to them when necessary. By the time the pots are filled with roots, and the autumnal rains render watering unnecessary, the basins are filled up by drawing the earth round each plant, at the same time pressing it firmly down, to prevent the wind from shaking them. A few of these plants in pots sometimes show flowers too soon, and to guard them from early frost, a leaf or two is broken down over them. On the approach of settled frost in December and January, all the pots are taken up and removed to a frame, pit, or shed, where they can be sheltered from the extreme severity of the winter, but have air when it is milder, and by this method a supply is preserved for the table in the hardest winters. To make broccoli succeed in pots, I find by experience, that it should be potted immediately from the seed bed. If it is transplanted oftener, the head or flower is both less in size, and runs much sooner after it forms. For the same reason, I never prick out or transplant the general crops; and as the temperature of our climate

Transplanting injurious.

climate does not suffer vegetation to go on briskly from October to March, by following this method, the heads of flower will remain a long time in a state of rest after they are formed without bursting, and heads from 6 to 7 inches diameter are the ordinary produce of our plants.

Third crop.

The seeds of the third crop are sown in a frame, or under hand glasses; and about the third week in October the plants become strong enough to remove, as in the two former crops. From this sowing, the best plants are selected for seed, and placed 3 or 4 under a hand glass according to its size; 3 however are sufficient, for they should not afterward be disturbed. They are gently watered and covered, till they have made fresh roots; after which air is plentifully admitted, treating them through the winter exactly like cauliflower plants. From the hints already given, it may be deduced, that these seedling plants should not only be placed in a part of the garden remote from every other variety of the cabbage tribe, but that no plant whatever of any variety, except it is wanted for seed, should be suffered accidentally to show a flower in the garden; and this business in the months of May and June, when two or three hot days often produce the effects of apparent enchantment, by suddenly bringing radishes, turnips, boorcole, cabbages, sea kale, and cauliflowers into bloom, requires very strict attention on the part of the gardener.

Seed plants.

General caution respecting seedling plants.

XI.

On some Exotics, which endure the open Air in Devonshire. In a Letter to the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. &c. By* A. HAWKINS, *Esq*[*]

SIR,

Exotics growing in the open air in Devonshire.

THOUGH I have no knowledge of the Horticultural Society, but through the medium of extracts in the last Monthly Review, (which informed me of its existence), yet, struck with your "Hints respecting the proper Mode of inuring tender Plants to our Climate," and residing in the very warmest part of England (the South Hams of Devonshire, of which I am a native), within view of an inlet of the sea, I am led to state to you some facts, that perhaps may not be wholly unworthy of notice.

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[*] Trans. of the Hort. Soc. vol. I, p. 175.

Camellia japonica.

In October, 1795, a camellia japonica was planted here among other shrubs in the open ground; it has stood every winter since, without the smallest shelter, thrives well, and has never had a branch or leaf injured by the weather; it is now about four feet high, the size of a gooseberry bush, but has not flowered.

Fuchsia coccinea.

Two plants of the fuchsia coccinea were planted about four years ago under a brick wall facing the south. At first the branches suffered by the frost, but they put forth new shoots in the spring, with much strength, and have flowered well every summer. During the last two years I was absent, but I understand, that only the extremities of the branches were injured, and they have always flowered in great perfection.

Solanum pseudocapsicum.

Some plants of solanum pseudocapsicum, or amomum Plinii, are also under a brick wall, (but not nailed against it) which have stood many years, and only a small part of the very extremities of their branches has been injured by frost.

Myrtles.

Myrtles of every kind (even the doubled blossomed and orange) do exceedingly well in the open ground, though the silver, from the richness of the soil, soon becomes plain[*].

Buddlea globosa.

The buddlea globosa likewise stands the climate; and some of the plants are ten feet high, spread wide, and make a handsome appearance. One of them is placed in a situation open to the north-east winds, where the sun cannot shine during the short days, yet it has stood there since 1794, and never had more than the extremities of the branches hurt.

American aloe.

About two miles from my house is the small seaport town of Salcombe, just between those two well known points, the Prawl and Bolt-head, the latter of which is in the parish whence this letter is written, a place that the sea washes on three sides. Perhaps of all spots in the British isles, Salcombe is the very first for climate and shelter. The celebrated Doctor Huxam used to call it the Moutpellier of England. In 1774, a large American aloe, only twenty-eight years old, and which had always stood in the open ground, without covering, flowered there; it grew to the height of twenty-eight feet, the leaves were six inches thick, and nine feet in length, and the flowers, on forty-two branches, innumerable.

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[*] I have seen myrtles, as far up the channel as Weymouth, both broad and narrow leaved, at least twelve or fifteen feet high, trained against walls in the open air, as jessamine commonly is. C.

Three leaved vervain.

Several plants of the verbena triphylla are growing at Salcombe in the open ground, and are now six feet high. I have not tried any of them myself; but as I expect to be more at home in future, than for some years past, I shall not fail to add this plant to those tender shrubs already growing around me.

Oranges and lemons.

Oranges and lemons, trained as peach trees against walls, and sheltered only with mats of straw during the winter, have been seen in a few gardens of the south of Devonshire for these hundred years. The fruit is as large and fine as any from Portugal; some lemons from a garden near this place were, about thirty-five or forty years ago, presented to the King by the late Earl Poulett, from his sister Lady Bridget Bastard, of Gerston; and there are trees still in the neighbourhood, the planting of which, I believe, is beyond memory. The late Mr. Pollexfen Bastard (uncle of the M. P. for Devon,) who had the greatest number of oranges and lemons of any one in this country, remarked above thirty years since (what tends to confirm your experiments), that he found trees raised from seed, and inoculated in his own garden, bore the cold better than oranges and lemons imported.

Trees from seed bear the cold best.

I have the honour to be,

Sir, your very obedient Servant,

A. HAWKINS.

Alston, near Kingsbridge, Devon,
December, 11, 1809.

XII.

On a new variety of Pear. *By* THOMAS ANDREW KNIGHT, *Esq, F. R. S. &c**.

Remarks on the ripening of the pear.

HAD the pear been recently introduced into England from a climate similar to that of the south of France, in which it had been found to ripen in the months of August and September, and to become fit for the dessert in the four succeeding months, it might have been inferred, with little apparent danger of errour, that the same fruit would ripen here in October, and be fit for our tables during winter; provided its blossoms proved sufficiently hardy to set in our climate. But had many varieties of this fruit been proved by subsequent experience to be capable of acquiring maturity before the conclusion of our summer, and in the early part of the autumn, without the aid of a wall, scarcely any doubts could have been entertained of the facility of obtaining numerous varieties, which would ripen well on standard trees to supply our tables during winter; for it would be very extraordinary, if the whole of our summer, and of our long, and generally warm autumn, would not effect that, which a part of our summer alone had been proved to be capable of effecting; nevertheless, though varieties of the pear abound, which bear and ripen well in the early part of the autumn, we possess scarcely any good winter pears, which do not require an east or west wall, in the warmer parts of England, and a south wall in the colder parts. This can arise only from the want of varieties; and I venture most confidently to predict, that (if proper experiments be made to form such varieties) winter pears, of equal merits with those which now grow on our best walls, will be obtained in the utmost abundance from standard trees; and that such pears may be sold with sufficient profit to the grower, on as low terms as apples are now sold, during winter: for I have had several opportunities of observing, that the fruit of seedling pear trees generally bears a considerable resemblance to that of their parent trees; and the experiments I have made on other species

We have no good winter pears that are standards.

But such might be obtained,

and the pears sold as cheap as apples.

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* Trans. of the Horticultural Society, vol. I, p. 178.

of fruits induce me to believe, that a good copy of almost any varieties may be obtained; and as I have more than once succeeded in combining the hardiness and vigour of the yellow Siberian crab, with the richness of the golden pippin, I do not doubt of the practicability of combining the hardiness of the swan's egg pear with all the valuable qualities of the colmar, or bezi de Chaumontel, and I consider the climate of England as peculiarly well calculated for the necessary experiments*.

Cheap fruits important in a national view.

I am disposed to annex some degree of importance to the production of abundant crops of fruit to supply our markets, at a moderate price, during the winter and spring; for it has been often observed, that great manufacturing towns have been generally more healthy in seasons, when fruits have abounded, than in others; and the same palate, which is accustomed to, and pleased with sweet fruits, is rarely found to be pleased with spirits, or strong fermented liquors: therefore, as feeble causes, which are constantly operating, ultimately produce very extensive effects on the habits of mankind, I am inclined to hope, and to believe, that markets abundantly supplied at all seasons with fruits would have a tendency to operate favourably, both on the physical and moral health of our people.

Attempts to effect this.

Under these considerations, I have amused myself with attempts to form new varieties of winter pears; and though my experiments are yet in their infancy, and I have seen the result of one only, and that under very unfavourable circumstances, I am induced to state the progress, that I have made, to the Horticultural Society, in the hope that others will join me in the same pursuit.

Experiment.

In the spring of the year 1797, I extracted the stamina from the blossoms of a young and vigorous tree of the autumn bergamot pear, which grew in a very rich soil; and I introduced, at the proper subsequent period, the pollen of the St. Germain pear, and from this experiment I obtained several fruits, with ripe seeds; I, however, succeeded in raising only two plants. One of these was feeble and dwarfish in its growth, as well as wild and thorny in its appearance, and I did not think it worth preserving. The other presented a much more favourable character; and I fancied that I could discover in it some traces of the features of its male parent. This plant afforded blossoms in the spring of 1808, but I had very unfortunately removed it from the seed bed, when it was fourteen feet high, in the preceding winter, and as it had never been previously transplanted it had retained but very few roots. Two of the blossoms, nevertheless, afforded fruit; which began to grow with rapidity as soon as the tree had emitted new roots, but this was not till late in the summer; and on the 8th of October the fruit was blown from the tree by a violent storm. The two pears were then very nearly of the same weight and size, each being somewhat more than eight inches in circumference, and in form almost perfectly spherical. Though bruised by their fall, the pears remained sound till the beginning of December, when they became sweet and melting, though not at all highly flavoured: their flavour was, however, better than I expected, for they were blown from the tree long before they would have ceased to grow larger, if the state of the weather would have permitted; and the autumn of 1808 was so excessively wet, that some St. Germain pears, which grew on a south wall in the same garden, were wholly without richness or flavour.

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* See Hort. Trans., vol. I, p. 30: or Journ. vol. XVIII, p. 159.

The new pear. The new pear very much resembled the St. Germain in the form of the eye and stalk, and the almost perfectly spherical shape is that which might have been anticipated from the forms of its parents. It will probably acquire a very large size under favourable circumstances; but removing from my late residence at Elton, I have been under the necessity of again transplanting the tree, and therefore I cannot expect to see its fruit in any degree of perfection till the year 1811. I have subsequently attempted to form other new varieties by introducing the pollen of the beurrée, crassane, and St. Germain pears, into the prepared blossoms of the autumn bergamot, the swan's egg, and Aston town pears; but I have not yet seen the result of the experiments. The leaves and habits of some of the young plants afford, however very favourable indications of the future produce.

Subsequent attempts.

The seeds In the preceding experiments I have always chosen to propagate from the seeds of such varieties as are sufficiently hardy to bear and ripen their fruit, even in unfavourable seasons and situations, without the protection of a wall; because in many experiments I have made with the view of ascertaining the comparative influence of the male and female parents on their offspring, I have observed in fruits, with few exceptions, a strong prevalence of the constitution and habits of the female parent; and consistently with this position the new pear I have described grew very freely in an unfavourable season, and in a climate in which the St. Germain pear, when its blossoms do not perish in the spring, will not grow at all, without the protection, and reflected heat, of a wall. I would therefore recommend every person, who is disposed to engage in the same pursuit, to employ the pollen only of such pears as the St. Germain, the d'Auche, the virgoleuse, the bezi, the chaumontel, the colmar, or bergamotte de pasques, and the seeds of the more hardy autumnal and winter kinds.

should be from hardy varieties,

and these trained to a west or south wall.

I would also recommend the trees from which the seeds are to be taken, to be trained to a west wall in the warmer parts of England, and to a south wall in the colder, so that the fruit may attain a perfect, though late, maturity. Every necessary precaution must of course be taken to prevent the introduction of the pollen of any other variety, than that from which it is wished to propagate, into the prepared blossoms.

A new plum.

I shall take this opportunity of pointing out to the Horticultural Society the merits of a new variety of plum, (Coe's golden drop) as a fruit for the dessert during winter, with which the public are not sufficiently well acquainted. Having suspended by their stalks, in a dry room, some fruit of this variety which had ripened on a west wall, in October, in the year 1808, it remained perfectly sound till the middle of December, when it was thought by my guests and myself, to be not at all inferior, either in richness or flavour, to the green gage, or drap d'or plum. I am informed by Mr. Whitley of Old Brompton, from whom I received it, that it bears well on standard trees.

XIII.

METEOROLOGICAL JOURNAL.

		PRESSURE.			TEMPERATURE.					
1812.	Wind	Max.	Min.	Med.	Max	Min.	Med.	Evap	Rain	
1st Mo.										
Jan. 6	N W	29·77	29·68	29·725	38	31	34·5	4		☽
7	N	30·05	29·68	29·865	38	33	35·5	—	·19	
8	N W	30·19	30·05	30·120	37	26	31·5	—		
9	N W	30·18	30·10	30·140	37	31	34·0	—		
10	Var.	30·18	30·13	30·155	36	32	34·0	—	·12	
11	N	30·13	29·89	30·010	38	32	35·0	—	·25	
12	N	29·89	29·79	29·840	42	32	37·0	—		
13	N W	29·98	29·79	29·885	40	35	37·5	—	1	
14	N W	30·08	29·98	30·030	41	33	37·0	·14		●
15	W	30·18	30·03	30·130	45	28	36·5	—		
16	W	30·20	30·17	30·185	39	32	35·5	—		
17	N	30·25	30·20	30·225	38	31	34·5	—		
18	W	30·25	30·15	30·200	43	34	38·5	—	5	
19	S W	30·15	29·97	30·060	47	36	41·5	—	3	
20	N W	29·97	29·88	29·925	43	29	36·0	—		
21	N W	29·88	29·86	29·870	40	28	34·0	·33		☾
22	N W	29·96	29·86	29·910	41	31	36·0	—		
23	N E	30·08	29·96	30·020	34	31	32·5	—		
24	N W	30·10	30·06	30·080	39	27	33·0	—		
25	W	30·06	30·00	30·030	41	39	40·0	—	3	
26	S	30·07	30·05	30·060	45	31	38·0	—		
27	S W	30·0[illegible]	29·87	29·960	47	31	39·0	·26		○
28	Var.	29·87	29·46	29·665	46	36	41·0	—		
29	S E	29·34	29·28	29·310	45	40	42·5	—	·12	
30	S	29·79	29·34	29·565	50	33	41·5	—	·24	
31	S E	29·79	29·79	29·790	48	41	44·5	·29	·10	
2d Mo.										
Feb. 1	S E	29·69	29·67	29·680	47	42	44·5	—	4	
2	S E	29·64	29·34	29·490	50	40	45·0	—	8	
3	S E	29·69	29·45	29·545	47	42	44·5	—	2	
4	S W	29·68	29·45	29·515	49	42	45·5	·32	1	
				29·899			38·0	1·40	1·29	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES

NOTES.

First Month. 6. Very fine morning: wet evening: the night stormy with much snow. 7. Snowy morning, stormy day. 9. Snow fell through the night, to about three inches depth. 10. a. m. Little wind, changing to S. W.: a thaw. London was this day involved, for several hours, in palpable darkness. The shops, offices, &c. were necessarily lighted up; but, the streets not being lighted as at night, it required no small care in the passenger to find his way, and avoid accidents. The sky, where any light pervaded it, showed the aspect of bronze! Such is, occasionally, the effect of the accumulation of smoke between two opposite gentle currents, or by means of a misty calm. I am informed that the fuliginous cloud was visible, in this instance, from a distance of forty miles. Were it not for the extreme mobility of our atmosphere, this volcano of a hundred thousand mouths would, in winter, be scarcely habitable! 16. A dripping mist. 18. Misty morning. 19. Very cloudy: large lunar halo: stormy night. 22. Snowy evening. 23, 24. Lunar halo. 28. Windy night. 29. Windy morning: wet evening.

Second Month. 2. Gloomy, with small rain at intervals. About half past 7, p. m. the wind rose and blew furiously from E. and S. E. about an hour and a half, the Barom. falling a quarter of an inch: abating afterwards, it rose again, and the night was stormy.

RESULTS.

Winds from the N. and W. to the time of Full Moon, then from the Eastward.

Barometer: highest observation 30·25 inches; lowest 29·28 inches; Mean of the period 29·899 inches.

Thermometer: highest observation 50°; lowest 26°; Mean of the period 38°.

Evaporation 1·40 inches. Rain, (including the products of snow) 1·29 inches.

The observations on the Barometer for the period, and the greater part of the Notes, were made at Stratford, by my friend John Gibson.

LONDON,
Second Month, 22, 1812.

L. HOWARD.

XIV.

METEOROLOGICAL TABLE for the Year, 1811,

Extracted from the Register kept at Kinfauns Castle, the Residence of Lord GRAY, *Three Miles from Perth, N. Britain, for the Year* 1811. *Communicated by his Lordship.*

1811.	*Morning*, 8 *o'clock* Mean height of		*Evening*, 10 *o'clock* Mean height of		Tot. rain fallen.	Number of days.	
	Barom.	Ther.	Barom.	Ther.	Inches.	Rain or snow.	Fair.
January.	29·87	33·19	29·55	33·32	1·45	14	17
February.	29·40	34·83	29·46	34·70	2·63	16	12
March.	30·04	39·80	30·25	39·70	0·90	9	22
April.	29·77	42·90	29·78	40·30	1·91	14	16
May.	29·84	50·03	29·83	47·70	3·12	20	11
June.	29·89	54·60	29·91	51·30	2·20	18	12
July.	29·99	58·01	30·00	55·40	2·86	14	17
August.	29·89	54·83	29·96	52·75	2·71	18	13
September	30·17	50·45	30·18	50·96	1·78	8	22
October.	29·77	49·54	29·78	49·59	4·41	25	6
November.	29·96	42·50	29·98	42·15	2·97	14	16
December.	29·78	35·03	29·80	34·85	1·80	15	16
Average of the year.	29·86	45·47	29·87	44·47	28·74	185	180

Kinfauns Castle is three miles almost due east from Perth. The house stands about ninety feet above the level of the River Tay, and probably only a few feet more above that of the sea.

Twickenham, 21 *Feb.* 1812.

*** The Author will be happy to receive an annual communication from his Lordship.

XV.

Remarks on some Electrical and Electrochemical Phenomena: by GEORGE JOHN SINGER, *Lecturer on Chemistry and Natural Philosophy.*

Electrical law of induction.

IN the last number of the Philosophical Journal, a correspondent, A. Z., concludes his remarks on Mr. Anderson's experiments by observing, "that the electrical law of *induction*, which Mr. Murray has pointed out," affords an explanation of the manner in which the decomposition, that occurs at every interruption of a metallic circuit in a fluid, is effected.

The term introduced by Mr. Davy.

The term *induction* was I believe unknown in electrical science, till introduced by Mr. Davy. That excellent chemical philosopher has indeed employed it very extensively, and appears to consider it as expressing the most important principles of electrical action.

Much obscurity occasioned by it.

The *general* application of this term to so many important phenomena has been productive of much obscurity. Its meaning, according to Mr. Davy's application, has never been clearly defined, and is by no means obvious; while its promiscuous employment in the explanation of various, and diametrically *opposite* effects, is contradictory, absurd, and unintelligible.—My present leisure will not allow me to speak of this subject so fully as may be requisite to its proper elucidation; but to counteract erroneous impressions, I shall briefly state some of those inconsistencies, to which I have referred.

Mr Davy uses it to express the action produced by approximation without contact.

Induction, as applied to *any* electrical phenomena, is unquestionably an objectionable term, as in its literal interpretation it expresses nothing analogous to any known electrical effect. By an attention to the writings and lectures of Mr. Davy, it may however be inferred, that his intention is to express that species of action, which results from the approximation, *without contact*, of an unelectrified to an electrified conductor; which has been called by Volta, and other electricians, electrical influence, and ascribed by Lord Stanhope to "the nature of an electrical equilibrium." It is well known to electricians, that an insulated conductor, when electrified either positively, or negatively, will alter the electrical state of any other body brought within a *certain* distance of it. This alteration will occur (though rather differently) whether the presented conductor be insulated or not; but it cannot be properly investigated, unless the conductor is insulated. As an example, the following well known but instructive experiment may suffice.

Experiment to

Insulate in a horizontal position a metal rod with blunt

or

Illustrate this action.

or rounded terminations, attach to each extremity of this rod a pair of pith balls, let the rod and its appendages be represented by A—B. Present the extremity A to a positively electrified surface, but *not* within its striking distance. Both pairs of balls will open; and, if examined, A will be found negative; B, positive. Remove the rod from the vicinity of the electrified surface, the balls immediately collapse, and every electric sign ceases. A proof, that no electricity has been communicated to it, and that the electrical appearances produced arise only from the unequal distribution of the natural electricity of the rod, during its approximation to the electrified surface; seeing that, as soon as this *cause* is removed, the effect it has produced immediately disappears.

No electricity communicated.

Experiment in which the electricity is communicated.

If the preceding experiment be varied, by bringing any part of A—B *within* the striking distance of the positive surface, each pair of pith balls will open as before, but they will be *similarly* electrified, being *both positive*; and this effect (arising from *communicated* electricity) will be permanent; for, on removing the rod A—B from its proximity to the electrified surface, the divarication of its balls will continue.

The same term therefore should not be applied to express different effects.

There are some variations of this experiment very important to electrical theory, but I mention here only the most simple facts, as my intention is merely to show the impropriety of applying the same explanation to *contrary* and opposite effects.

An electrified surface may act in two ways:

From what has been stated it will be evident, there are *two* methods by which an electrified surface may excite electrical effects in other bodies.

by approximation,

1st. By approximation. In this case the electrified surface loses *none* of its intensity, and the previously unelectrified body becomes electrical only in consequence of the *unequal distribution* of its natural electricity.

and by communication.

2nd. By direct communication. In this case the original surface *loses* part of its intensity, and the body previously unelectrified becomes electrical in consequence of an *alteration* in the *quantity* of its natural electricity.

The different effects of these two methods of electrification are

Different effects of these.

A. By the first, a positive surface may be employed to electrify another body positively, or negatively; or it will electrify the same body at *once* positively, negatively, and neutral in different parts of its length; but in this latter case, the electrical effects will last only *during the approximation.*

B. It is not possible to produce these effects of approximation, unless the bodies be *separated* by a *nonconductor,* the *resistance* of which is sufficient to prevent the passage of electricity from one to the other.

C. By the second method a positive surface can only communicate positive electricity; and *vice versa.* But these *communicated* effects are *permanent* after the *separation* of the bodies.

D. The effects of communicated electricity can only take place by the *actual contact* of conducting bodies, or where the intervening medium does *not completely resist* the passage of electricity.

These two sets of phenomena distinct.

No two series of phenomena can I think be more distinct than the preceding; their conditions of action are directly contrary, and it must be consequently obvious, the same explanation cannot apply to both.

Mr. Davy has applied the term induction to both,

Mr. Davy has however for a long time applied the term *induction* to some of these opposite cases of electrical action; for instance, the Leyden jar, and the insulated rod; distinct cases of approximation: the spiral tube, luminous word, electric spark, &c., decisive instances of communicated electricity.

and attempted to explain the action of the pile on the same principle.

In the Bakerian lecture for 1807, an explanation of the voltaic battery was attempted on similar principles; and this explanation has been repeated every succeeding season in the lectures at the Royal Institution. It assumes, "that, with regard to electricity of such low intensity, "water is an *insulating* body." An assumption, which has been decisively contradicted by the experiments of Mr. de Luc, which prove, that the battery is a *conducting* column. Yet Mr. Davy continues to speak of *induction,* and electrical polarity; and A. Z. appears to think it capable of explaining the interrupted circuit; where the whole column is indisputably a *conductor,* and where the phenomena depend on the *circulation* of electricity *through its whole length!* or in other words, on the transmission of electricity from *wire* to *wire* through *water*.

The existence of a positive and negative point at each interruption of the circuit not proved.

I have already stated, that the chemical changes produced in fluids by voltaic electricity, at every interruption of the metallic circuit, are no proofs of the existence of a positive and a negative point at each of those interruptions; so long as we have *no evidence*, that the *chemical effects* are produced by *opposite* electrical states, and not by the peculiar modifications of a current. We have no such proof; nor have we yet electrometrical demonstration, that the opposite extremities of every wire in an interrupted voltaic circuit are oppositely electrified, though Mr. Davy has very recently said "it would be easy to show this." Mr. de Luc's analysis* is the only instance of an accurate examination of the electrical state of the wires, compared with their chemical effects; his conclusions are however strikingly opposed to those of electrical energy; they have been long published, and are not yet controverted.

Mr. de Luc's analysis.

Let us suppose however for a moment, that the opposite electrical states are essential to the chemical effects. Can we have recourse to *induction* to explain them? Certainly not, unless by *induction* we mean communicated electricity, circulation of electricity, or current of electricity; for these alone express what we see and demonstrate in the experiment; and are indeed employed (certainly incautiously) by the same philosophers who speak of electric energy.

Induction an improper term.

If induction be strained to express these terms, it cannot possibly be applied to the opposite series of phenomena, viz. those produced by approximation only; where no current is produced, but merely a temporary disturbance of the electrical equilibrium effected.

Hence I think it fair to conclude, that induction, far from an electrical law, is a term ill suited to express any electrical action; a term which it would be contradictory and absurd, to apply to all the varieties of this influence; and which is in any case objectionable, as involving the assumption* of a principle of action not proved to exist.

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* See Journal, vol. XXVI, pp. 113 and 241.

The fallacy of this principle in one of its most popular applications, the hypothesis of the Voltaic battery, I shall consider in a future letter.

3, Prince's Street, Cavendish Square,
February 15th, 1812.

XVI.

On some new Varieties of the Peach. By T. A. Knight, *Esq., F. R. S., &c*[*].

Experiments for obtaining new varieties of the peach.

IN the Transactions of the Horticultural Society of 1807, I have mentioned some experiments I had made with the hope of obtaining new and early varieties of the peach[†], which might prove better calculated for our climate, than those which have been imported from the southern parts of Europe: and as the character of some of the plants that I have raised affords a fair prospect of success, I have thought the following account sufficiently interesting, to induce me to send it to the Horticultural Society.

Management of the trees for blossoms and seeds.

In efforts to obtain new varieties of fruits of other genera, I have had reason to conclude from the success of former experiments, that the trees, from blossoms and seeds of which it is proposed to propagate, should have grown at least two years in mould of the best quality; that during this period, they ought not to be suffered to exhaust themselves, by bearing any considerable crop of fruit; and that the wood of the preceding year should be thoroughly ripened, (by artificial heat when necessary) at an early period in the autumn: and if early maturity in the fruit of the new seedling plant is required, I think, that the fruit, within which the seed grows, should be made to acquire maturity within as short a period as is consistent with its attaining its full size, and perfect flavour: those qualities ought also to be sought in the parent fruits, which are desired in the offspring; and the most perfect and vigorous offspring will be obtained, of plants as of animals, when the male and female parent are not closely related to each other*.

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[*] Trans. of the Hort. Soc. vol. I, p. 162.

[†] See Journal, vol. XVIII, p. 195.

Experiment. The varieties of the peach, from which I first propagated, were the large French mignon, and the little red nutmeg, using the stigmata of the former, and the pollen only of the latter. The trees of each variety had been removed early in the spring of the preceding year (1801) from pots of moderate size into others which were very large, and were filled with mould of the most favourable quality, that I could compose; and in these pots the plants had grown with excessive vigour. The aid of artificial heat was employed in the spring of 1802, to enable the wood and blossoms of each plant to acquire the most perfect state of maturity in the succeeding autumn; and during winter the pots were defended from severe frost, that the minute fibrous roots of the plants might be wholly preserved; and as the spring approached the trees were kept in as low and equal a temperature as possible, that the powers of life, in the plants, might not be prematurely excited into action, or in any degree uselessly expended. Nevertheless, owing to the wood and buds having acquired maturity early in the preceding autumn, and an accumulated excitability from long rest and cold, the blossoms began to swell rapidly on the first approach of spring; and very early in March it became necessary to place the trees in the forcing-house, the blossoms being so far advanced, as to be subject to some danger from frost.

As soon as the blossoms had fallen, the fruit was ripened under every advantage of heat and light, that I could command, the glass having been taken off every favourable hour, during the last swelling of the fruit, to admit the solar rays, without its intervention. Three French mignon peaches only were suffered to remain on each tree, and six of these, (which attained the greatest state of perfection), afforded me eight plants in the succeeding spring. The plants were two years old when mentioned in a former

The trees bore at three years,

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* See Horticult. Trans. of 1807, Part. I, p. 30, or Journ vol. XVIII, p. 189.

communication, and I then inferred, from the rapid change observable in the character of the leaves and general growth, that they would bear fruit, as they subsequently did, when three years old.

and the fruit ripened early.

Of the new varieties thus obtained three are very early; but I have not had an opportunity of comparing their time of ripening with that of the earliest old varieties. For the red nutmeg peach did not succeed at all in my garden, and the blossoms of the early Anne were wholly destroyed by the unfavourable weather of the spring of 1807 and the following year. Two of the new varieties, however, ripened ten days before the royal George peach, and three weeks before the red Roman nectarine, which grew on the same wall, and adjoined the seedling trees; and therefore I conceive these not to be much later varieties than their male parent, which they strongly resemble in colour, and in the form and character of their leaves: but their fruit is much larger, many having exceeded 7½ inches in circumference. The fruit of each of the new varieties is soft and melting, and very readily quits the stone; and I thought the flavour of one of them quite equal to that of any peach which my garden produced. In their leaves and fruit, every tree forms a perfectly distinct variety, and even where the same stone contained two plants, they bear very little resemblance to each other.

Each tree a distinct variety.

Different in hardiness.

In the present spring I exposed all the seedling plants without any covering, to ascertain the comparative degrees of hardiness of their blossoms; and in this respect I found them to differ very widely. The blossoms of two of the varieties appear, however, to be very hardy, and promise an abundant crop of fruit, though the season has been more than usually unfavourable; and I have had the pleasure to observe, that the best peach is one of the most hardy.

Hope of obtaining standard peach trees.

The success therefore of the first and of the only experiment, of which I have fully seen the result, on this species of fruit, has fully answered, and indeed exceeded my hopes; and I entertain little doubt that the peach-tree might, in successive generations, be so far hardened and naturalized to the climate of England and Ireland, as to succeed well as a standard in favourable situations. It is my wish to try the

Experiments intended.

the effects of propagating successive generations alternately from the open wall, and from the hot-house, and of introducing the pollen from the open wall to the blossoms of the hot-house, with the hope of obtaining varieties which will be at once hardy and early. The peach does not, like many other species of fruit, much exercise the patience of the gardener, who raises it from the seed; for it may always be made to bear when three years old, and there is something in its habits which induces me to believe, that it might be made to bear at two years old. I will not venture to decide, whether it might not possibly produce fruit even at the end of a single year; and therefore, as the improvement of this, and other species of fruit, and adapting varieties of them to our climate, presents an ample and interesting field for experiment, I trust that I shall not labour in it alone.

The peach bears soon.

Cautions.

In prosecuting such experiments, I would recommend the seedling peach trees to be retained in pots, and buds from them, only, to be inserted in older trees; for their rapid and luxuriant growth is extremely troublesome on the wall, and pruning is death to them.

XVII.

On the Areolites, that fell near Lissa in Bohemia, on the 3d of September, 1808: *by Mr.* Reuss, *Counsellor of Mines*[*].

Account of some meteoric stones that fell near Lissa in Bohemia.

THE account we have of this fall of stones was collected on the spot, by the mayor of the place, four days after it happened. Farther information was collected afterward by Mr. Merkl, counsellor of state, who has lodged an official statement of it in the chancery.

Lissa is a small town of the circle of Buntzlau, four miles W. N. W. of Prague, and as many S. S. W. of Jungbuntzlau; two miles N. of Benatek; two E. of Altbuntzlau and Brandeis; and two W. of Nimbourg.

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[*] Ann. de Chim. vol. LXXIV, p. 84. Abridged by Mr. Tassaert.

The place described.

The country where these stones fell is a plain, extending southward to the Elbe. The soil is a poor dry sand, fit only for rye; and the rocks that are found there are of an argillaceous gritstone impregnated with iron. The field on which one of the stones fell was a very loose sand, that had just been ploughed up; yet the stone penetrated into it only four inches. The second fell in an adjoining field, the soil of which was rather harder, and more clayey; yet it penetrated only four or five inches. The third fell in a small wood of fir trees, on a sandy ground covered here and there with turf, and likewise made an impression four or five inches deep. The fourth, which was found about two thousand paces from the village of Straton, weighed two pounds and a quarter; but one corner was broken off, without the inquirer's being able to learn how. The third stone, that fell in the wood, weighed five pounds nine ounces and half, though all its angles and edges had been damaged. The direction in which these stones fell was from the north.

Circumstances accompanying the fall.

The circumstances, that accompanied this fall of stones, were nearly the same, as have been observed in other places. On Saturday the 3d of September, 1808, at half after three o'clock in the afternoon, a loud explosion was heard, which all the witnesses compared to a discharge of several pieces of cannon, succeeded by a noise like that of firing by companies, or the roll of drums. This noise continued a full quarter, or even near half an hour. The sky, which had been very clear, appeared covered as with a slight gauze; yet the rays of the Sun penetrated easily through this sort of thin mist. The night preceding had been fine, calm, and very clear; the weather had been fine the whole day, except about noon, when a few drops of rain fell, but the clouds dispersed; and about three o'clock in the afternoon the heat was considerable, and the weather heavy *(lourd)*.

No person saw these stones fall, so that we know not whether they were black, or red, or smoking, when they fell; but some reapers, who took up one as soon as it had fallen, found it as cold as the stones around. It did not soil the fingers, and none of them had any smell of sulphur. No person observed any lightning, or luminous meteor; neither

rain

rain nor wind was noticed; and no one felt any of that uneasiness or oppression, that indicates electricity.

The stones described.

These aerolites, like all others, were of a mixed substance. They are of a light ashen gray colour, fine grained, traversed in all directions by little veins, and interspersed with little disseminated globules. Their specific gravity is 3·56. Brought near a compass, they cause the needle to move through an arc of 8°. When reduced to powder, globules may be extracted from it by the magnet.

Analysis of the aerolite of Lissa, by Mr. KLAPROTH.

Analysis of the stone.

Though all the external characters of this stone of Lissa lead to the presumption, that it must contain the same substances as those, which chemical analysis has demonstrated in other meteoric stones; yet the subject is too interesting, to allow us to neglect an accurate examination of every fresh specimen, for the purpose of discovering how or in what proportions it may differ from those already analysed.

Mr. Reuss having sent me a sufficient quantity of this stone, I subjected it to the following analysis.

Iron extracted by the magnet.

a. 200 grains were reduced to powder; and from these 29 grs were extracted by the magnet. These were in small ramified particles. The remaining powder still contained some small shining metallic points, which might be considered as sulphuret of iron, as sulphuretted hidrogen gas was obtained from it on treating it with muriatic acid.

The metal dissolved in muriatic acid.

b. The 29 grs of metal were dissolved in muriatic acid by the assistance of a gentle heat. Sulphuretted hidrogen was evolved, and the liquid at first appeared foul and milky. Five grains of the powder of the stone, that had adhered to the globules of the iron, remained undissolved. The acid liquor had not the emerald green colour, that the solution of meteoric iron commonly has; but was simply greenish, which indicated but a small portion of nickel. In order to oxide the iron completely, I added nitric acid to the boiling liquor, precipitated the oxide of iron by ammonia, and filtered. The ammoniacal liquor was of a pale blue colour. On evaporating it to dryness, and heating it red hot in a platina crucible, a little yellowish gray residuum was left. This residuum, dissolved in nitric acid, formed a green liquor, which became blue on supersaturation with ammonia. This liquor, evaporated anew, yielded an apple-green salt, which was heated red hot, to decompose the nitrate of ammonia. The residuum, which was black, was again redissolved in nitric acid, and filtered, to separate a blackish matter acquired from the platina crucible. The nitric solution, precipitated by carbonate of soda, yielded a pale green carbonate of nickel.

The stony matter treated with potash,

c. The 171 grs of stony powder left by the magnet in experiment *a*, with the five grains of earthy residuum, were heated in a silver crucible with twice their weight of potash. This mixture became blueish by fusion. Diluted with water, the lixivium assumed a greenish hue. The filtered alkaline liquor remained clear when neutralized by nitric acid. The solution was evaporated to dryness, the salt redissolved in water without leaving any residuum, and on adding nitrate of mercury nothing but a white precipitate was obtained. This trial, which had been instituted for the detection of chrome, did not afford the slightest indication of this metal; though some have asserted, that it exists in aerolites.

lixiviated,

and treated with muriatic acid.

d. The powder of the stone having been well lixiviated, and treated with muriatic acid, dissolved in it by the assistance of heat. The liquor was evaporated to dryness, and the residuum redissolved in water, and filtered. The silex, well washed and heated redhot, weighed 83·5 grs.

The solution precipitated by carbonate of potash.

e. The muriatic solution, freed from silex, was precipitated cold by carbonate of potash. The alkaline liquor, separated from the brown precipitate, was subjected to ebullition, and mixed with as much carbonate of potash, as was requisite to precipitate it. The precipitate consisted of carbonate of magnesia.

The precipitate treated with caustic potash.

f. The brown precipitate formed by the carbonate of potash in experiment *e* was boiled with caustic potash. The alkaline liquor, supersaturated with muriatic acid, and then, precipitated by carbonate of potash, yielded a white flocculent precipitate; which, after being heated redhot, weighed 2·5 grs; and was found to be alumine, on treating it with sulphuric acid.

Dissolved in nitric acid, and precipitated by succinate of iron.

g. The brown precipitate, which had been treated with potash, was dissolved in nitric acid; and, after the too great excess of acid had been saturated by soda, the liquor was precipitated by succinate of iron, and the precipitate heated redhot: then, after adding the oxide of iron obtained in experiment *b*, and dropping on it a little oil, it was heated redhot in a close vessel. The oxidulated iron in this state weighed 80 grs, answering to 58 of metallic iron.

The liquor precipitated,

h. The liquor separated from the iron was precipitated by carbonate of potash while boiling. A greenish white precipitate of carbonate of magnesia was obtained, which was added to the carbonate of magnesia of experiment *e*, and exposed to a strong red heat. This changed the colour reddish, and the magnesia weighed 48 grs. Being treated with sulphuric acid diluted with water, half a grain of oxide of manganese was separated from it.

and the precipitate treated with sulphuric acid.

The solution evaporated and redissolved.

i. The sulphuric solution was evaporated to dryness, and the salt redissolved in a great deal of water. Some silex separated, which, after calcination, weighed 2.5 grs. On evaporating the solution, small acicular crystals were obtained, which were sulphate of lime, and weighed 3 grains, equivalent to one grain of lime.

k. The remainder of the solution afforded nothing but sulphate of magnesia, the quantity of base in which was only 44 grains, when the weight of the silex, lime, and oxide of manganese was deducted. The colour of the sulphate of magnesia still tending to green, the presence of a little nickel was to be presumed. Accordingly the salt was redissolved in water, and the nickel precipitated by a stream of sulphuretted hidrogen. The oxide thus obtained was mixed with that of experiment *b*, and exposed to a strong red heat; after which it weighed 1·5 gr., answering to 1 gr. of nickel.

From the results of this analysis it appears, that 100 parts of the aerolite of Lissa gave

Component parts of the stone.

Iron	*g*		29
Nickel	*k*		0·50
Manganese	*h*		0·25
Silex	*d* *i*	41.75 1·25	43
Magnesia	*k*		22
Alumine	*f*		1·25
Lime	*i*		0·50
Sulphur and loss			3·50
			100

I have supposed, that all the iron in the aerolite was in the metallic state. Formerly such as could be extracted by the magnet was alone so reckoned, the rest being considered as oxide of iron. But as there is no sign of oxidation in this aerolite recently fallen, it is evident, that the shining points, which did not adhere to the magnet, were pyrites, in which the iron was contained originally in the metallic state.

Proust's hypothesis.

The hypothesis of Propst, that aerolites are products of our globe, expelled from the polar regions to fall nearer the equator, is founded on the total absence of oxigen.

That of others.

This circumstance, however, is equally favourable to the opinion of those, who suppose them to be thrown from the moon; since astronomers deny to this satellite an atmospher containing oxigen, and saturated with watery vapours like that of our globe.

The stone could not have been formed in the atmosphere.

But it is certain, that the total absence of this principle completely refutes the opinion of those, who believe, that these aerolites are formed in the regions of our atmosphere; since the particles of iron and martial pyrites would not remain even so short a time without a commencement of oxidation.

This analysis of an aerolite so recent affords a fresh proof, that they are all nearly of the same nature; as the preceding account by Mr. Reuss shows, that they have all been projected from higher regions. But the naturalist, who would honestly build only on certain facts, must not be ashamed to confess, that he is ignorant of their origin.

Meteoric stone of Stannern.

I shall add, that a powdered specimen of an aerolite, which fell near Stannern, in Moravia, on the 22d of May, 1808, and of which consequently I know not the external characters, has been sent me. This stone would be a striking exception to all the aerolites known, since, from my analysis of a very small quantity, it would appear to be a decomposed basaltes. It is to be wished therefore, that the analysis should be repeated with a piece of the stone in its entire state, possessing all the characters necessary to prevent suspicion respecting it*.

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* This stone has been analysed by Vauquelin, see Journal, vol. XXV, p. 54; and no doubt the pretended specimen sent to Klaproth in powder was an imposition. C.

XVIII.

XVIII.

An Answer to the Observations of Dr. PEARSON, *(see our last Number) on certain Statements respecting the Alkaline Matter contained in Dropsical Fluids, and in the Serum of the Blood. By* ALEXANDER MARCET, *M. D. F. R. S., one of the Physicians to Guy's Hospital.*

To Mr. NICHOLSON.

SIR,

The question not placed in it's proper light.

ALTHOUGH I feel extremely disinclined to engage in any public philosophical controversy, especially when the object is to vindicate statements, the truth of which any common observer may easily ascertain by experiment; yet, as there are some points in the above communication, which do not place the question in its proper light, and might mislead those, who have not the opportunity of referring to the original documents, I have thought it necessary, to offer in return a few observations.

Its proper state.

The state of the question is simply this: All chemists have for a long time agreed that the blood, and probably all the animal fluids, contain, together with various neutral salts, a certain portion of alkali not combined with any acid. This alkali has generally been considered as being soda, although a few chemists had also noticed traces of potash in some of these fluids. Dr. Pearson, on the contrary, in examining various kinds of animal substances, and especially of expectorated matter, was led to conclude that the whole of the uncombined alkali contained in the animal fluids was potash, and that they did not contain uncombined* soda in any proportion whatever.

Soda the only uncombined, alkali in the fluids.

In analysing the fluids of dropsy, I was naturally led to inquire into this question; and the result obtained induced me to conclude, that the only uncombined alkali present in the blood, or other animal fluids, was soda; and that the indications of potash, which, by applying the test used by Dr. Pearson, I was able to detect in these fluids, were owing to the presence of this alkali in a state of combination with the muriatic acid.

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* By the expression *uncombined,* I mean not combined with acid.

This proved in two ways.

The experiments I adduced in evidence were of two kinds; some of them showing that the uncombined alkali was soda, and others that it was not potash.

Salts obtained from different animal fluids.

Portions of saline matter being procured from various animal fluids by evaporation and incineration; and brought by subsequent redissolution and evaporation to a crystalline state, crystals of determinate forms were obtained, some of which appeared to consist exclusively of sub-carbonat of soda, some of muriat of soda, and others of muriat of potash; but none could be detected, which appeared to contain any carbonat of potash.

The uncombined alkali treated with acetic acid and alcohol.

Other similar portions of the saline matter being treated with acetic acid, in order to bring any uncombined alkali present to the state of acetat; and alcohol being added with a view to separate these acetats, the residue of this alcoholic solution appeared to consist almost* solely of acetat of soda; while, on the other hand, potash was found in the residue left undissolved by the alcohol.

Potash in a state of combination.

In these various trials the presence of potash, in a state of combination, was proved by the tests of oximuriate of platina and tartaric acid, both of which form precipitates with potash, and not with soda.

The uncombined alkali soda.

The uncombined alkali, on the contrary, was shown *not* to be potash by the last-mentioned tests failing to indicate the presence of this alkali; while, on the other hand, it was proved to be soda, by the action of nitric acid, which, in combining with it, formed crystals of a rhomboidal, instead of a prismatic figure.

Dr. Pearson's objections examined.

I shall not enter into the particulars of these operations, because they are minutely related in the communication which has given rise to this discussion; but I shall now rapidly examine the principal objections which Dr. Pearson has made to the above conclusions.

1st objection.

Dr. Pearson's first ground of complaint is, that, instead of showing his conclusions to have been erroneous; that is,

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* A trace of potash was detected in the alcoholic solution; but it must be remembered, that alcohol, however rectified, will take up minute portions of muriat of potash, or indeed of almost any other soluble salt.

I conceive, instead of following him step by step in his inquiry, I have contented myself with exhibiting my own experiments and conclusions. But I beg to observe, that the object of my inquiry was not to repeat Dr. Pearson's experiments, but to examine dropsical fluids; and that, if in the course of my analysis I met with results which militated against his conclusions, it could not be reasonably expected, that, in stating these results, I should think it incumbent upon me to wade through his laborious researches on the various forms of sputum or expectorated matter. I might indeed have abstained altogether from referring to his labours; but I thought it due to him, as a philosophical inquirer long known in the chemical world, to point out such similarities or discordances of results, as occurred in our re-respective experiments; thus referring the matter to the decision of physiologists, and showing, that there was no wish on my side to overlook the authority of former inquirers.

In endeavouring to analyse the various objections brought forward by Dr. Pearson, I am so often at a loss to understand his meaning; and I must add, so much embarrassed by the obscure and inaccurate manner in which he has stated some of my own proceedings, that it would be a task equally fruitless and laborious to follow his steps closely. I must, therefore, as much as possible, select those objections which are of a specific nature, and may be answered by an appeal to experimental evidence. Such is, for instance, the argument which he employs, no less than three times, (once in support of his own experiments, and twice with a view to invalidate my inferences), on the effects of alcohol and acetic acid, which argument is founded upon his belief, that acetat of soda is *not* soluble in alcohol; and that it is *not* a deliquescent salt; two palpable errors, which half a grain of this salt, and a few drops of alcohol, with no other apparatus than a watch glass, would have enabled him to rectify.

Argument from the effects of alcohol and acetic acid:

from the minute quantity of saline matter used:

But the objection, which recurs the most frequently, and that upon which the greatest stress is laid, is the minuteness of the quantities of saline matter subjected to experiment. It would appear, that Dr. Pearson questions whether a few grains of saline matter may be expected to yield results similar to those which would be obtained from larger quantities; whether, for instance, the same inferences might be drawn from rhomboidal crystals of a minute size, as from similar crystals of larger dimensions; or whether experiments tried upon an ounce or two of my dropsical fluids, may be brought into competition with those which he performed upon two or three pints of his ropy sputum.

Such a scepticism, I must own, I have myself never entertained. I have always thought on the contrary, that the chemical properties, which belonged to a particle of matter, were exactly similar to those, which would be found to belong to a whole mountain of the same substance; that a rhomb of only one hundredth part of an inch might be characterised by its form as distinctly as one a hundred times larger[*]. But I carry the point still farther, for I go the length of believing, that many experiments of research may be wonderfully facilitated by analysing upon a small scale; that a great deal of convenience, of economy, and sometimes even of accuracy, may thus be gained; and that in some instances we may even obtain new and unexpected powers of inquiry by operating upon small quantities[†].

Experiments on a small scale vindicated.

Instances of

Thus, were it not for the assistance of minute or microstheir aid to science.

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[*] Thus I have no hesitation in maintaining, that unless it be proved, that nitrate of potash may crystallize in rhombs, my conclusions respecting the particular point in question, would stand upon that evidence alone; or that unless it be shown, that carbonate of potash may crystalize in cubes, my inference respecting the presence of muriate of potash stands uncontroverted.

With regard to my attempt at expressing centesimal parts of grains, which is, with some apparent reason, noticed as an instance of singular pretention to accuracy, I beg to observe, that I have never actually attempted to weigh smaller quantities than decimal parts of grains; and whenever smaller fractions have been expressed, they have arisen from a conversion of those numbers to some general standard.

[†] I would also observe, while upon this subject, that there is a degree of neatness gained, by reducing the scale of operations, which is often incompatible with processes in the large way. Thus I have never found it necessary, in analysing, to introduce among the enumeration of contents, "a little dirt," as some old-school chemists have been in the habit of doing.

copic observation, a great number of important facts, which have enriched chemistry within the last 20 years, would, in all probability, have remained undiscovered; and this country might not have obtained that first rank in philosophical chemistry, to which it has but lately been raised, and which it had long held in other departments of science.

Is it necessary that I should specify particular instances? Can any philosopher, attentive to the progress of analytical chemistry, overlook so many discoveries, in which neither furnace nor forge, nor subterraneous laboratories have been concerned, in which a watch glass, a blow pipe, and a few drops of chemical reagents, have been all the instruments required? Were not, for instance, the analyses of the Iceland springs, by Dr. Black, (the same eminent philosopher to whom Dr. Pearson appeals, as an authority against microscopic observations), performed upon quantities of saline matter, of astonishing minuteness? Surely Dr. Pearson cannot have forgotten, that it was by the accurate examination of only a few grains of matter, that the nature of no less than five kinds of urinary calculi has been ascertained, and their discrimination rendered easy and certain; that the nature of diamond has been established; that no less than four new metals have been discovered in the crude ore of platina; that the similarity between all the meteoric stones has been proved; that the identity of the chemical agencies of electricity, whether excited by the common machine, or by the voltaic battery, has been demonstrated; that in a neighbouring country the formation of crystals has been explained upon systematic principles; that among us a new and wonderfully accurate instrument of crystallography has been invented; and above all, that the metallic bases of alkalies, those extraordinary bodies which Nature had hitherto concealed under an impenetrable disguise, have at last been brought to light. Let it be remembered as one of the most glorious circumstances of that discovery, that it was by examining mere atoms of these substances, that their properties were first ascertained; and that when, in consequence of subsequent improvements in the mode of obtaining these bodies, they were procured in larger quantities, and their general properties were reexamined, no error was discovered, and no important information was added to that which had originally been gained from microscopic quantities.

Experiments on a large scale not without their use.

It is far from my intention, however, to contend, that on some occasions, new and important facts may not be brought to light by means of processes conducted upon an extensive scale, which would not admit of being reduced to a small compass. I only mean to assert, that such instances are comparatively but rare; and that no philippic against the examination of small objects; no appeal to old Masters; no slight upon modern improvements, ought to deter chemical inquirers from adopting methods, which some of our contemporaries have employed with so much utility and success.

Dr Wollaston inaccurately joined with the author.

Among other inaccuracies in the critique which has given rise to these remarks, my paper on dropsical fluids has been represented as being the joint work of Dr. Wollaston and myself; for which supposition there was no other authority but a note in the paper in question, in which I acknowledged my obligations to Dr. Wollaston for the information and assistance, which I have, on this and other occasions, derived from his kindness. I need not say how highly I should have been flattered by such an association; but I think it due to him to state, not only that he had no share in the general inquiry, but that he did not even see the paper in question previous to its publication.

Expressions not used by the author cited so as to be taken for his.

I cannot refrain from noticing, among Dr. Pearson's remarks, another kind of licence, which appears to me still less warrantable. I allude to the practice of quoting in italics or placing between inverted commas, words or phrases which have not been used, and to seize upon them as a subject of ridicule. This is the case with some proposed *elegant* changes, and with my supposed recommendation to transfer chemistry to the "fireside of the drawingroom"; expressions which I have not used, and yet upon which Dr. Pearson has thought proper to be extremely jocular.

I have not only farther to add, that should Dr. Pearson again write upon the subject, I shall not easily be induced to resume the controversy. I am sorry, therefore, to find it intimated at the conclusion of his paper, that he proposes to continue his observations in your next number; and as it appears, that these intended remarks are meant as a return for the notice which I have taken of his papers, I regret the more that he should take so much trouble. For praise, when used as the vehicle of irony, is the worst kind of censure. The discovery of truth ought to be the only object of philosophical discussion. There are, doubtless many errors in my humble attempts at chemical analysis; but unless Dr. Pearson points out those errors, or brings forward new facts connected with my inquiries, I confess I had much rather he would not again honour them with his notice.

Truth should be the sole object of philosophical discussion.

"Quicquid id est, timeo Danaos, & dona ferentes."

I remain, Sir, &c.
ALEXANDER MARCET.

XIX.

On the supposed Presence of Water in Muriatic Acid Gas. In a Letter from a Correspondent.

To Mr. NICHOLSON,

SIR,

Experiment to show, that muriatic acid gas contains no water.

HAVING seen in your Journal for the last month a statement of Mr. Murray's relative to the presence of water in muriatic acid gas, and being present at a lecture of Mr. Davy's at the Royal Institution on the 7th of February, in the course of which he repeated it with very different results, I was induced to repeat it also. The mode of Mr. Davy's experiment was so very unexceptionable, I determined to adopt it; it was as follows, viz: having obtained ammoniacal and muriatic acid gasses pure, I introduced them into a retort, which was previously exhausted. They immediately combined and formed muriate of ammonia. Then, having cleared a part of the neck, for any condensation of fluid that might occur, I applied heat, until all the salt was sublimed into the neck of the retort, and did not obtain a particle of moisture. I then removed some of the salt through the atmosphere into a dry tube, and applied heat, and obtained vapour. I repeated this again, having suffered the salt to be exposed for a few minutes to the atmosphere, and obtained water again, so that Mr. Murray might have obtained in this way to thrice the weight of the salt he employed. Now in my opinion this clearly proves, that the water which Mr. Murray obtained, was from the atmosphere, and not from either of the gasses, as he thinks. It will be unnecessary to offer any observations on an experiment evidently so inaccurate.

I am, Sir, your most obedient,

A. B. C.

SCIENTIFIC NEWS.

Caledonian Horticultural Society.

Prizes proposed by the Caledonian Horticultural Society.

As the improvement of horticulture is an object of considerable importance to the comforts of life and its innocent enjoyments, we insert the following summary of the series of prizes proposed by this society for the present year, as well as those of more general scope.

They are, under the first head, the silver medal, for the best early cucumber, grapes, spring brocoli, Brussels sprouts, winter lettuces, seedling polyanthuses, and early melon, to be shown on the second Tuesday of March or May: the best melon, forced peaches, cauliflowers, on the second Tuesday of June: the best seedling pinks, on the second Tuesday of July: the best twelve sorts of gooseberries, on the first Tuesday of August: the best peaches and nectarines from the open air, apricots, green gage plums, jargonelle pears, seedling carnations, and home-made wine without any foreign materials but sugar, on the second Tuesday of September: the best six kinds of apples and of pears, heads of late brocoli, forced sea-cale, and forced asparagus on the second Tuesday of December.

II. The production of *new* or *improved* varieties of fruits, culinary vegetables, or flowers.

1. For the best new apple, adapted to the climate of Scotland, raised from seed. Ten years to be allowed. Gold medal

Prizes proposed by the Caledonian Horticultural Society.

medal and twenty guineas. 2. For the best new pear, raised as above. Ten years to be allowed. Gold medal and twenty guineas. 3. For the best new peach or nectarine, raised as above. Six years to be allowed. Gold medal and ten guineas. 4. For an improved variety of the Dutch currant, raised from seed. Five years to be allowed. Gold medal. 5. For the best new and productive early melon. Gold medal. 6. For the best new early cucumber. Gold medal. 7. For the best new strawberry, raised from seed. Four years to be allowed. Gold medal. 8. For the best new sort of early potato, *without blossoms*, raised from seed. Five years to be allowed. Gold medal.

III. Communications, &c. (The gold or the silver medal to be awarded by the committee, according to the value and importance of the communication.)

[It is expected, that all communications will be founded on *actual* experiments.]

1. On the best method of improving the sorts of brocoli already cultivated, and of saving their seeds genuine in this climate. 2. The best method of cultivating and of forcing sea-cale. 3. The best treatise on orchard fruits adapted to the climate of Scotland, with lists and descriptions of the different kinds,—their habits of growth, &c.—their synonyms or local names; those for the table, and those for kitchen use. 4. The best treatise on the culture of the Dutch currant for wine. 5. The best mode of preventing or curing the mildew upon different fruit-trees and other vegetables. 6. The best mode of preventing or curing the canker in fruit-trees, &c. 7. The cheapest and most effectual mode of preserving fruit trees on walls from the effects of late spring frosts. 8. The best mode of destroying the blue insect, breeding in the crevices of the bark of apple-trees, and causing them to canker and die, chiefly on those trees imported from the London nurseries. 9. The best method of destroying wasps, woodlice, earwigs, &c., infesting wall-fruits. 10. The best mode of preventing the depredations of the turnip-fly. 11. The best method of preventing worms in carrot, in cauliflower, and brocoli roots. 12. The best mode of destroying the wire-worm.

13.

13. The best mode of destroying the pine bug, the brown scale, the white bug, the aphis or green fly, the chermes, the red spider, the thrips; or any other insect infesting hot-houses, pits, melon and cucumber frames, &c. 14. The best means of increasing the quantity of manure, and the best mode of applying it to different crops. 15. The best means of bringing into a bearing state full grown fruit-trees (especially some of the finest sorts of French pears) which, though apparently in a very healthy and luxuriant condition, are yet in a state of almost total barrenness. 16. The best account of a Scotch Kitchen Garden, or of a Scotch orchard. 17. The best method of preparing opium in this country; and the most advantageous manner of cultivating the white poppy for this purpose. 18. For the best essay on preventing the curl in potato. 19. For the best essay on destroying or preventing caterpillars on gooseberry bushes and fruit trees. 20. For the greatest quantity of asparagus, planted upon sandy land near the sea, and manured with sea weed only; not less than a quarter of an English acre. 21. For the greatest quantity of sea-cale, planted on the same kind of land, and manured with sea weed; not less than ten falls English measure. 22. For the greatest number of pints of strawberries produced from the smallest extent of ground, not less than a quarter of an acre.

Prizes proposed by the Caledonian Horticultural Society.

It is requested, that each article brought in competition may have attached to it a particular motto, and be accompanied with a sealed letter referring to such motto, and mentioning the competitor's address.

Communications, either on the above subjects, or on any other topic connected with horticulture, may be addressed to Mr. T. Dickson, Leith Walk, or to Mr. P. Neill, Old Fish Market Close, Edinburgh. ***

Geological Society.

Supposed native lead.

At the meeting on the 21st of February, an extract of a letter from Mr. J. R. Jones of Holywell to the President was read, giving an account of a specimen, presented by him to the society, of supposed native lead, found in a bed of granite in the neighbourhood of Holywell.

Submarine volcano.

An extract of a letter communicated by the hon. Henry Gray Bennet, describing a submarine volcano, which made its appearance on February the 1st, 1811, off the island of St. Michaels, in the Azores.

Cornish oxide of tin.

The reading of a paper by W. Phillips, Esq., entitled "a description of the Oxide of Tin, the production of "Cornwall; of the primitive crystal and its modifications "including an attempt to ascertain with precision the admeasurement of its angles mechanically, by means of the "reflecting goniometer of Dr. Wollaston; to which is "added a series of its crystalline forms and varieties;" was commenced.

The native oxide of tin appears to have been found in almost every district of Cornwall, and in the opinion of Mr. P. is by no means peculiar to the primitive rocks of that country. Particular crystalline modifications of this substance characterize particular veins.

Alluvial depositions of tin of considerable extent and depth have been found in several parts of Cornwall, which appears to be the only part of Europe, in which this metal occurs under these circumstances. The peculiar variety called wood tin has hitherto been met with only in these beds, or stream-works, as they are termed in the country; and these have also furnished the only specimens of gold hitherto found in Cornwall.

Among the specimens of tin in the collection of Mr. Phillips it may be observed occurring in granite, in mica slate, and in other varieties of schist, accompanied by chlorite, tourmaline, calcareous spar, schiefer spar, topaz, chalcedony, quartz, fluor spar and chlorophane, yellow copper ore, blende, arsenical pyrites, and wolfram.

Chemical lectures.

Mr. Singer will commence his course of Lectures on Chemistry, at the Scientific Institution, on Tuesday the 3d of March; they will be continued on each succeeding Tuesday, at eight o'clock in the evening.

Sections of Aquatic Plants

Fig 1

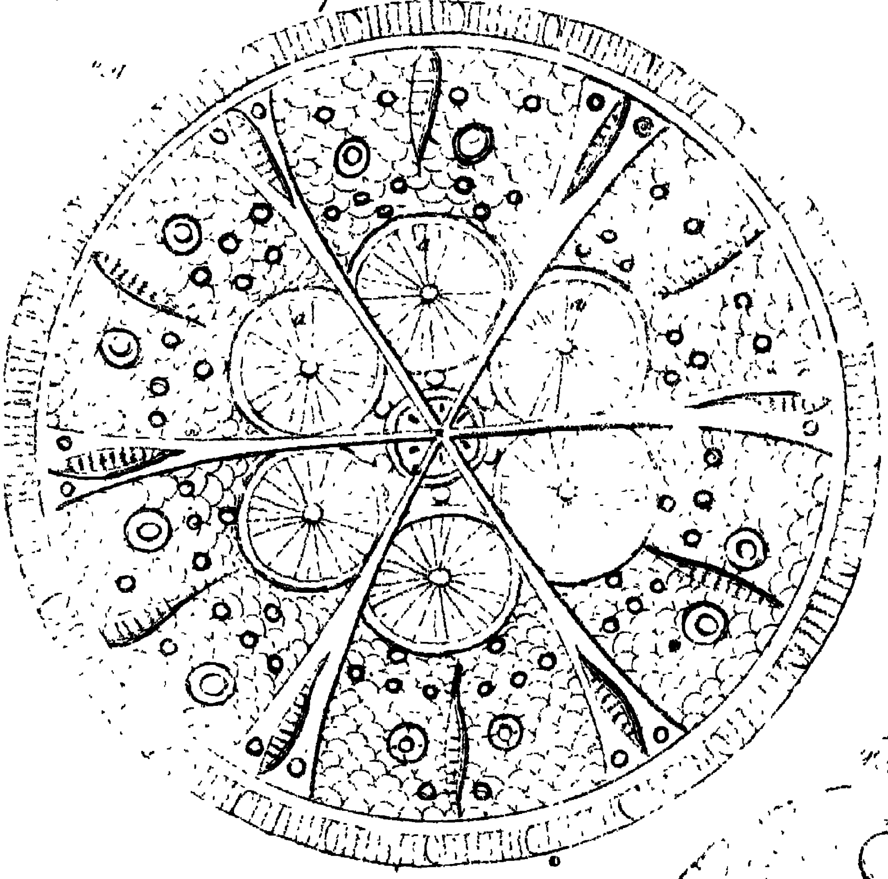


Fig 2

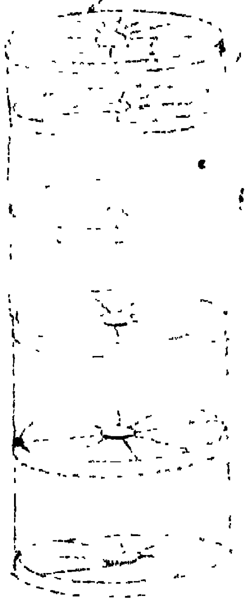


Fig 3

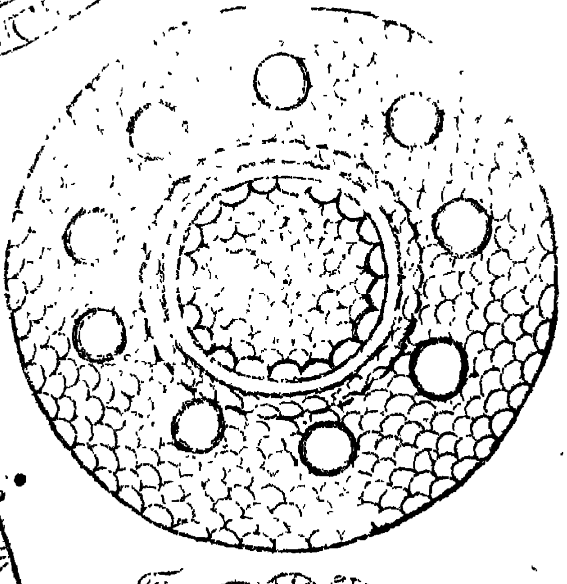


Fig 4

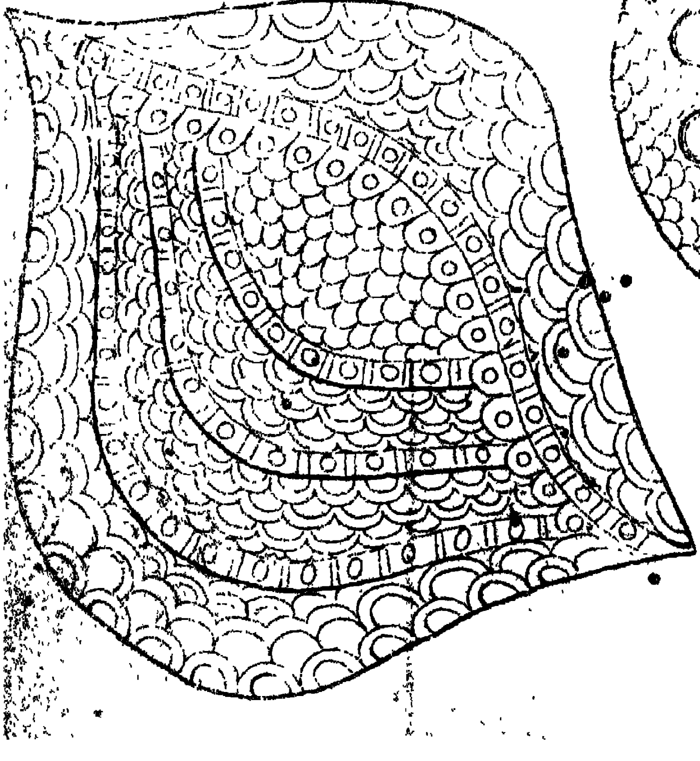
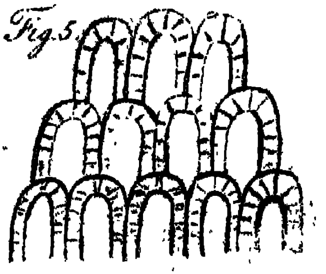


Fig. 5.



Dissections of Marine Plants

Fig. 1

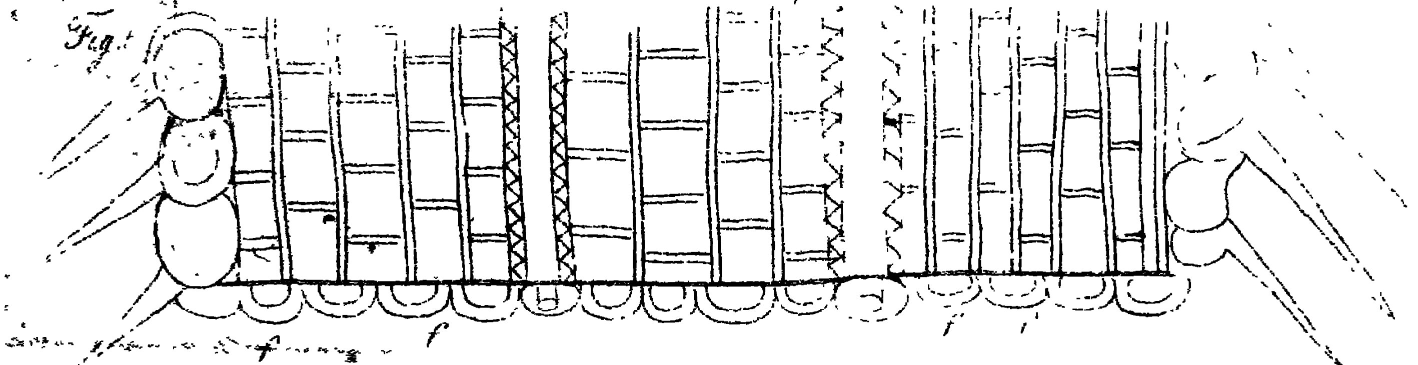
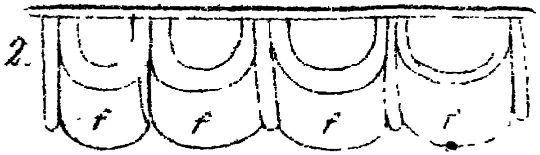


Fig. 2.



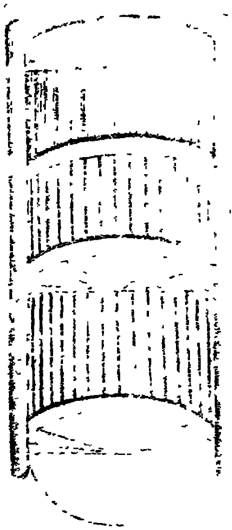


Fig 3.

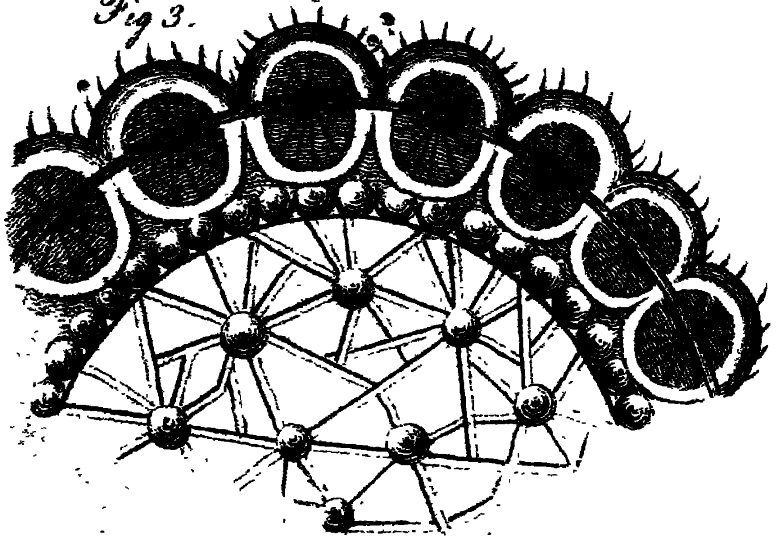


Fig. 4

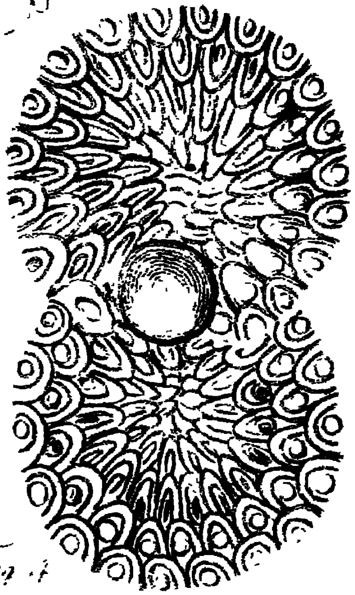
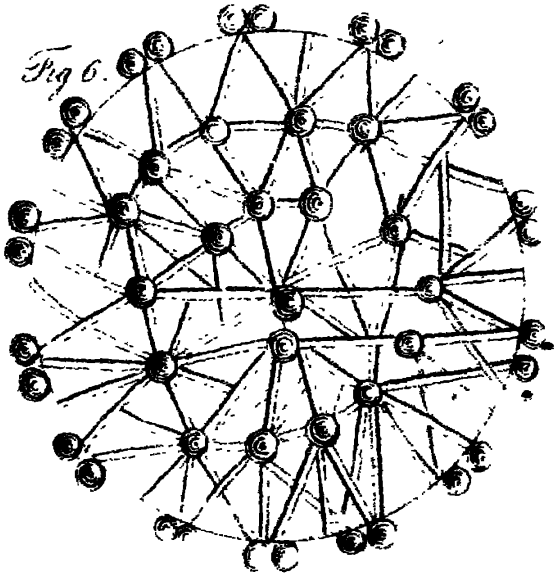


Fig 6.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1812.

ARTICLE I.

On fresh-water Plants. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

The necessity of arranging dissections.

I Ventured to observe in my last letter, that, without keeping to a certain classification in plants, when their dissection was pointed out, especially when no prints attended the work, it was almost impossible to comprehend it, though perfectly well instructed in the subject intended to be described. The natural system, which arranges certain figures together, and finds a similitude unmarked by the eye till the knife has dissected and pointed it out, is perfectly unconnected with any other selection. Thus, the grasses, water-grasses, ferns, mosses, fresh-water plants, and cryptogamia, are all so completely unlike, that to give their parts in a cursory manner, is to render them almost unintelligible. As long as the several divisions are arranged in the usual course of rind, bark, wood, &c., they may all be compared with the formation of trees: and so long as the wood is placed

placed in regular vessels, surrounded by the clear albumen, and containing the spiral vessels; and as these cylinders are sprinkled all over the interior, and the interstices filled with pith: they are known to be herbaceous, annual, or semiplants, and their formation is understood, and they will still bear a sort of comparison with trees and shrubs. But when the whole arrangement is overturned, and plants are found possessing parts unseen before, as in the fresh-water plants; or when in dissection the usual assortment of matter can no longer be recognized, and neither wood, bark, spiral vessels, &c. can any more be found, as is certainly the case in the marine plants; then it is absolutely necessary, to begin again the description, and by strict examination and trial discover anew the different uses, to which these parts can be applied.

Monsieur du Thouars' work.

It is this cursory method of giving every different sort of plant together, which makes the work of Monsieur du Thouars so difficult to understand: but it must be considered, that he delivered most of his book in lectures to his pupils, and probably exemplified them in the very best manner by living specimens, which would explain them to his hearers, though not to his readers.

Marine plants.

As I intend to show in this letter the formation of fresh-water plants, and in my next letter marine plants, it will not be amiss to draw first a sort of comparison between them, at once to prove the necessity of the arrangement just specified. Marine plants have no vessels. The whole is formed, whether thick or thin, by *blebs*, which allow of no communication one with the other: so that, if only a part of a fucus is drawn out of the water, this part dies, the rest not being capable of conveying to it any of its moisture. Each bleb has I believe a pore, but very difficult to find. They have no peculiar air vessels, though much air is mixed with the liquid within; which, not being confined in vessels, merely fills the blebs, and is retained by the cuticle of each surface. The only vessel to be found in a fucus, ulva, &c., is the line of life. They have no spiral wires, and of course no divisions of bark, wood, &c.; and appear in short to differ so much from every other plant, that their means of nourishment and existence must be wholly of another kind, and

Fresh-water plants.

Stem of the water lily.

and require a very different process. But fresh-water plants are still capable of comparison with others, since, on dissecting them, you directly perceive the same sort of matter, as rind, bark, wood, &c. The line of life forms the centre, or meanders in it, and the air vessels are generally ranged next to it. I shall give the stem of the water lily as the first specimen (see fig. 1, Pl. VII). I have said, that the line of life is in the centre, where it forms a very thick circle. The whole is then divided into six parts by the bastard vessels, which pass from the centre to the circumference. Six large cylinders fill up a part adjoining the line. These are the air vessels, (see *a a*); and the rest of the space is occupied by the pith, sprinkled with wood vessels. The circumference is bounded with a few rows of bark, enclosing some inner bark vessels, and a rind surrounds the whole. This, with the figure, will give a general idea of it.

Its air vessels.

Hairs.

Wood vessels.

Buds.

But it is necessary to give some description of the air vessel, which is really curious in its formation. It is a large cylinder, divided at every half inch of its length with a thin texture of pith: but lest this should not be sufficient to prevent insects from entering it, and choking up the vessel, as soon as the plant sinks in the water, a quantity of hairs, which are placed in circles in the interior, rise, and, meeting in the centre, not only aid to keep out the water, but run through every insect, that ventures to approach. I have often caught insects threaded on the hairs, but they are soon washed off. Sometimes the hairs remain in a horizontal position; but in general they rise and fall with the water. This, exciting my curiosity, made me anxious to apply to the solar microscope for the discovery of the mechanism, which regulated the motion of the hairs: and I found, that, though there was no spiral wire in the other parts of the plant, it was to be found in the hairs.* The formation was simple, and merely caused by the contracting and dilating of the wire, as the hairs were drawn up, and ranged against the side of the cylinder, or placed themselves horizontally, their points meeting in the centre. The wood vessels are to be distinguished from the buds by their inferior size and circular shape. The buds (as in most annual plants) proceed from the root; and of course have a stem shooting thence, and never showing any leaves. The flower bud must therefore be growing all the time the stem is shooting, and does not appear till that stops, it is the same with the leaf, which has also a peculiar stem from the root, and but one leaf to each.

Not useful to classification.

It is impossible, that the formation of water plants in general can be of any use to classification, or to the selection of a natural method, as I once hoped it would be. Complete water plants indeed are outwardly known by those well acquainted with their general appearance, and with the classes. But the half water plants are culled from every genus, even those springing in the dryest land, yet varying, it should seem, from their species by growing in the water. What a light does this throw on the uses of the different parts of a plant! It is the water, which appears to operate on the interior formation of the stems, peduncles, and vessels of the leaf. Nor does this alteration seem to effect the conformation of the fruit or flower, which are the same as in the rest of the species. Not even the seed shows to the eye any difference, but into this latter fact I mean to make a farther inquiry. In the veronica beccabunga, where two or three are taken from many of the species growing in tolerably dry soils, the whole formation of the stem is altered; see fig. 3. Instead of a great portion of bark, eight or ten large air vessels supply its place. The rind is completely formed of cylinders of air and water divided. Instead of a wide row of wood, it has one line and eight circular vessels of the same, half wood, half clear albumen. It has indeed its spiral lines within these, and its line of life meandering in the pith: but this is all in which the stem resembles the usual veronicas, which are in the interior all alike, the water variety excepted: and what is curious, the anagallis and scutella growing only in boggy ground, not so wet as the usual situations of the beccabunga, has fewer air vessels, and more wood. The sisymbrium nasturtium, menyanthes trifoliata, ranunculus aquatica, potamogeton pusillum, and many others equally deviate from their species, and deserve the name of half-water plants, being more or less varied. That these should possess the spiral is not to be wondered at, since the leaves, being raised much above the water,

Half water plants.

require it to turn themselves, and expose their upper surfaces to light and heat; which when they lie on the water (as in absolute water plants) is not necessary. The leaves also of the plants just mentioned, the veronica and menyanthes, have in their peduncles many air, and but one set of wood vessels; but the most curious part is a sort of perforation in the bottom of the peduncle, which is formed between the air vessels and lower cuticle to contain air, and support the leaf stem upright above the water.

Air vessels forming to accommodate to their element.

May we not therefore conclude, that the air vessels are only intended to support the stem upright in a different element, and to raise it above, or depress it below the water, as the situation of the plant required? Could I but procure a plant, that had by change of climate become a sort of water plant (which I should suppose is possible) it would then be easy to see, whether the air vessels would form themselves to accommodate their structure to the element in which they reside. I cannot but be persuaded I have seen changes as great: will not a tree by degrees change its time of shooting? will not a plant often seek a more agreeable soil if near it? But time may enable me to show this in a more conspicuous manner. I am so fearful of advancing a single step beyond what specimens will absolutely disclose, that I would far rather leave a fact unaccounted for, than pass beyond what dissection will really justify or bring proofs of.

Difference between a real water grass,

As it will not be possible, on account of the numerous figures, to give all I have to say on fresh-water plants in this letter, I shall conclude by showing the difference between a real water grass and a half-water one, or that which at one time of the year lies on water. The former is entirely composed of rows of air vessels, which between every three or four have a row of bark studded with wood vessels, very small, and as usual half wood, half albumen, but no spiral vessels. When the flower shoots, it is in thick threads from the root; slipping up between the leaf and its outward cuticle, as in all grasses. This thread is the line of life, with some wood surrounding it, and is a fresh proof, if it was required, that the female proceeds from this line, and the male from the wood. But the half-water grass is very dif-

and a half-water grass.

ferently contrived, the upper face exactly resembles common grass, but to support it on the water it has long cylinders, which are merely of a loosened skin of that kind, which permits not water to penetrate. These are the air vessels, and support the grass perfectly dry on the water, where it swims, and defies both rain and wind, (see Pl. VIII, fig. 1 and 2, where *ff* are the air vessels; fig. 4, Pl. VII, being the water grass). By this it may be seen how gradually the plants approach to the state of perfect water plants, and if any other proof is wanted, the potamogeton lucens, which has generally a double stem, would show it. This grows constantly in the water; it is small, and requires few wood vessels to bring it support. It is almost wholly composed of vessels of air, one wood vessel being between each row; but it has a long circular stem in the interior, bounded by the line of life, and with a deep border of wood, up which the buds pass, as in all the fern and potamogeton genera, though some have several instead of a single one. To this specimen I shall add a sort of scirpus growing always in water, having its air vessels next the rind, as in figs 3 and 6, Pl. VIII, and merely threads tying the wood vessels in the middle, one to the other. The air vessels resemble in form those of the water lily, and are very different from the air cylinders in half-water plants, which I should before have mentioned. These are divided into compartments, as in Pl. VIII, fig. 5, *e d e*, which represents the veronica beccabunga, being half air vessels, half wood, so that the air enters only alternately; and they have no hairs as in the water lily, the wood part being wholly filled up.

Dissection of a scirpus.

Dissection of air vessels of the veronica.

No perspiration in plants.

The perspiration of aquatic plants was supposed to be uncommonly copious. I have so often troubled the public with the proofs of there being no perspiration in plants, that I shall not here repeat them, but merely give an explanation of the cause of the quantity of air found around almost all fresh water plants. There is in the bark juice (that is, the blood of the plant) a glutinous matter, which when moved catches the air in bubbles, and will continually enclose it, and cover the whole leaf and plant with vesicles of air, by which means it is prevented from sinking, let the rain beat ever so hard against it, or the wind attack it. I have seen the bubbles of air formed by a brisk blowing wind cover a plant, till it appeared as if rolled in diamonds: but when I have drawn off the upper cuticle of the leaf, and subjected it to the solar microscope, not a single aperture could be discovered, and the marks proved as usual to be only the indentations of the pabulum seen through the cuticle.

To this I shall add the parts that belong to each different sort of plant, which will elucidate the subject, and show the confusion which must arise from not properly discriminating them.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

Different Parts of each various Sort of Matter found in the different Stems of Plants, and arranged in the manner they pass from the exterior to the interior Parts.

Structure of the stems of plants.

In trees in general.—Rind, bark, inner bark vessels, wood, spiral vessels wound round the inner wood vessels, line of life, pith; bud proceeding from the nearest line of life, whether in stem or twig.

Fir trees.—Leaves cover instead of rind and bark, and thicken by degrees, having the inner bark vessels within the leaves: a thick row of wood, resembling the common wood, placed as a screen to guard the new wood from the deleterious effect of the juices: hard wood, line of life, pith: bud as usual in trees.

Shrubs.—Rind, bark, inner bark vessels, wood, spiral vessels, line of life, pith: bud as usual in trees.

Herbaceous Plants.—Rind, bark, inner bark vessels, wood, line of life within the pith, wood generally in rows, in number according to the length of the season: buds shooting from the interior of the pith, of course forming the line of life.

Annual and semiplants.—Rind, bark, inner bark vessels, wood in circular vessels, half wood half clear albumen, scattered in the pith, with the spiral vessels within the wood, and having the line of life within the pith.

Fresh-water plants.—Rind, air vessels, wood in scattered vessels all over the pith, being partly wood, partly albumen, but having no spiral vessels within, but the line of life in the centre, in a thick line; buds proceeding from the root.

Half-water plants.—Rind, bark, air vessels disposed in it with inner bark vessels; wood, either in rows or scattered vessels with albumen, and spiral vessels within; line of life meandering in the pith.

Marine Plants.—Rind, the rest vesicles of a glutinous matter, with a pore to each, but no communication from one to the other. Though an appearance of stalk, yet formed exactly the same as the rest of the plant, and without any vessels or lines except the line of life difficult to find, but in the fructification most plainly appearing.

The other parts of the cryptogamia will be given in my next letter.

II.

On the Zigzag Motion of the electric Spark. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Zigzag course of the electric spark.

ALLOW me, through the medium of your valuable Journal, to communicate a supposition on a point, that seems to have been withheld entirely from public discussion: I mean the zigzag appearance of the electric spark passing from one body to another, as from a positive to a negative, &c. Partial to the science, but limited in experiment, or you might have had enough to prove a belief in the idea now formed; the only account I have ever heard at lectures was, that its own rapidity of motion condensed the air to such a degree, that it had to move from a solid, as it were, to a less dense medium, which seems to me impossible. My supposition is, that the fluid passes in a more direct line, according to the best or worst conducting substances presented to it. Our atmosphere, being a compound of oxigen

Owing to the

gen &c., presents at once to the spark flying off the machine at least four known gasses, all, I have not the smallest doubt, differing in their conducting powers, were they separately tried. This point being ascertained, the phenomenon is at once accounted for: the fluid flies to the next best conducting gas from a worse, as it would from different portions of matter. I could advance more on this, only fear occupying too much space at the expense of more valuable communication than this from

compound nature of the atmosphere.

Your most obedient servant,

I. PHŒNIX.

Liverpool, the 16th of Jan. 1812.

III.

Abstract of a Paper on Fermentation: by Mr. Gay-Lussac*.

It is fully demonstrated by the experiments of Lavoisier, as well as by those of Messrs. Fabroni and Thenard, that to produce alcoholic fermentation requires the concurrence of a saccharine matter, and a peculiar ferment of an animal nature. The circumstances favourable to fermentation have been long noticed: and it appears to be at present admitted, that it may be begun and continued without the assistance of any foreign matter, even of oxigen gas. It has been ascertained in fact, that, when the yeast of beer is introduced with sugar and water into a vessel, so as to fill it entirely, fermentation takes place in it in the same manner as in the open air: and hence it has been inferred, that the fermentation of the must of grapes, saccharine fruits, and grain, would take place, like that of sugar and yeast, without the contact of oxigen gas. But to render this inference legitimate, it must be presumed, that the ferment contained in fermentable substances is of the same nature as that of yeast. Mr. Thenard, to whom we are indebted

Saccharine matter and a ferment requisite to vinous fermentation.

May be carried on without air,

in all cases as some have supposed,

which requires the identity of the ferment.

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* Ann. de Chim. vol LXXVI, p. 245. Read to the Institute Dec. the 3d, 1810.

for an excellent paper on fermentation*, has accordingly adopted the opinion, that the ferment was in all cases identical. The experiments I have made have led me to a different opinion; and the principal object of this paper will be to show, that the fermentation of grape must cannot take place without the assistance of oxigen gas. Hence it follows, that the ferment of the grape is not of the same nature as yeast; or rather, that they are not both in the same state.

This opinion erroneous.

I was led to this inquiry by an examination of the processes employed by Mr. Appert for preserving vegetable and animal substances†. I had observed with surprise, that grape must, which had been kept unaltered a whole year, began to ferment in a few days after being poured into fresh vessels. It is in this way Mr. Appert prepares sparkling wines [*vins mousseux*] at all seasons of the year. This fact led me to suspect, that the air had some influence on fermentation, and suggested to me the following experiments.

Preservation of animal and vegetable substances.

The air influences fermentation.

I took a bottle of grape must that had been kept a year, and was perfectly limpid; poured it into another bottle, which I corked tight; and exposed it to a temperature from 15° to 30° [59° to 86° F.]. In a week's time the must had lost its transparency; fermentation had taken place in it; and it was soon converted into a vinous liquor, sparkling like the best champagne. A second bottle, that had been kept a year, like the preceding, but was not exposed to the contact of air, gave no signs of fermentation, though placed in the most favourable circumstances for producing it.

Experiment with grape juice.

Fermentation excited by the contact of air.

I then took this bottle of grape must, cut it pretty deeply round the neck with a file, inverted it in a mercurial trough, and then broke off the neck, without suffering the must to come into contact with the air. One portion of the must I passed through the mercury into a jar containing a small quantity of oxigen gas, and another portion into a jar perfectly void of air. The first fermented in a few days:

The same shown by another experiment.

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[*] Ann. de Chim vol. XLVI, p. 291. See Journ. vol. VII, p. 33.

[†] These processes, which are extremely simple, consist in putting the substances to be preserved into bottles, corking them very close, and then exposing them to the heat of boiling water for a longer or shorter time. See the instructions published by Mr. Appert.

the second gave no sign of fermentation in forty. On absorbing by potash the carbonic acid gas evolved during the fermentation of the first portion, a very little residuum was left; consequently the greater part of the oxigen gas I had added was absorbed.

Oxigen absorbed.

These results evidently prove, that must kept a long time cannot ferment without the contact of oxigen gas. But to obtain still greater certainty on this point, I analysed with Volta's eudiometer the air found in several bottles of must, that had been kept a year, and found in them no oxigen.

Farther confirmation of this.

I proceeded in the same way with the juice of gooseberries and grape must recently prepared, which had been exposed in well-corked bottles to the heat of boiling water, and obtained precisely the same results.

Sim with fresh juice.

It is very remarkable, that, when a fermentable juice, which has been kept a long time, is poured into another vessel, so that it would ferment from having been exposed to the contact of the air, it may readily be deprived of this property, by exposing it anew, in bottles closely corked, to the heat of boiling water. By this operation we perceive it loses its transparency, and afterward lets fail a slight sediment. During the fermentation of a very limpid juice a sediment is also deposited: but there is this difference between them; that of the latter is capable of exciting fermentation, but that of the former no longer enjoys this property.

Fermentation destroyed by heat and exclusion of air.

Sediment.

From these several results I have considered it as very probable, that grape must recently obtained would not ferment, if the grapes were pressed without the contact of air. Accordingly I took a jar, into which I introduced some small bunches of grapes perfectly whole; inverted it under mercury; and filled it five times following with hidrogen gas, in order to expel the smallest portions of atmospheric air. I then bruised the grapes in the jar by means of an iron rod, and exposed them to a temperature of 15° or 20° [59° or 68]. Twenty-five days after no fermentation appeared; though must, to which I had added a little oxigen, had begun to ferment the first day, and in a short time after fermented very briskly. In these last two experiments I observed, that the oxigen was almost wholly absorbed; but

Grape juice pressed out without the contact of air will not ferment;

but I cannot say whether it combined with carbon, or with hidrogen. I obtained a quantity of carbonic acid gas equal in bulk to a hundred and twenty times the oxigen gas I had added to the grape must; whence it is evident, that, if oxigen be necessary to the commencement of the fermentation, it is not to its continuance; and that the greater part of the carbonic acid produced is the result of the mutual action of the principles of the ferment and those of the saccharine matter.

unless the grapes were ripe, and then slowly,

except oxigen be present.

In another experiment of the same kind as the preceding, a fermentation commenced at the expiration of twenty-one days, but the grapes were in a very advanced stage of ripeness: and, besides, a portion of the same must, placed in contact with a little oxigen, had fermented in six and thirty hours after it had been prepared. Hence it is farther evident from this experiment, that oxigen gas is singularly favourable to the developement of fermentation.

Oxigen equally promotes the fermentation of animal substances.

This action of oxigen on fermentable juices is observable also in animal substances. I have seen bottles containing beef, mutton, fish even, and mushrooms, prepared at Mr. Appert's; and a month after these different substances were found to be perfectly good. On being exposed to the air, they soon putrefied, as fresh animal substances would have done. On the contrary, if they were replaced in bottles after having been in contact with the air a few hours only, and were then exposed to the heat of boiling water, they would keep a very long while. If however the bottles were badly corked; and particularly if the heat were not sufficiently prolonged; and all the oxigen contained in the bottles were not absorbed, putrefaction soon came on. In fact, by analysing the air in the bottles in which these substances have been well kept, we may convince ourselves, that it no longer contains any oxigen; and that the absence of this gas is consequently a necessary condition for the preservation of animal and vegetable substances.

Vegetable and animal substances preserved by occasional heating, even when air is present.

On reflecting, that putrefaction and fermentation never develope themselves instantaneously, I conceived, from the preceding results, that vegetable or animal substances might be preserved, without being deprived of the contact of air, by exposing them occasionally to the heat of boiling water. Accordingly I took some cow's milk, gooseberry juice, and a solution of gelatine, and exposed them to the boiling heat of water saturated with salt, at first daily, afterward every other day.

Two months after all these substances were perfectly good. The butter, that had collected on the surface of the milk, was very sweet, only it was a little harder than fresh butter; and the milk appeared a little thinner than before the experiment. I need not say, that some milk, gooseberry juice, and jelly, which I kept by way of comparison, soon altered.

Urine keeps long if air be excluded.

Urine, which is known soon to putrefy, and from acid, which it is at first, to become alkaline, will keep a long time in vessels closely stopped, when it has scarcely been in contact with the air: it retains its transparency, acidity, and smell; and no ammoniaco-magnesian phosphate is deposited, though sometimes uric acid separates. When urine is left in contact with a small portion of air, it absorbs its oxigen pretty readily, and then the decomposition stops: but if a sufficient quantity of air be present, a great deal of carbonate of ammonia is formed, and ammoniaco-magnesian phosphate is almost always deposited with the phosphate of lime. The decomposition of urine therefore, as we see, is not analogous to fermentation; since the latter, when once it has begun, goes on without the assistance of oxigen gas.

Essential difference in ferments,

Returning to fermentation, and considering, that sugar and the yeast of beer will ferment without the contact of air, while the must of grapes has not this property; we are forced to admit, that there is an essential difference between yeast of beer and the ferment of the grape. Yeast is solid, and nearly insoluble in water: ferment, on the contrary, in the state in which it is found in fermentable fruits, is liquid; or, if it be solid, it must be very soluble in their juices. It appears to me however, that it may be solid in a great number of substances, but in a peculiar state, and different from that of beer yeast. Still it is very possible, that there is but one ferment, and that its difference from the yeast of beer is to be ascribed only to a little oxigen. In this view it would be analogous to indigo, which is capable of oxidation and disoxidation.

but perhaps merely from oxigenation.

Fermentation still mysterious.

Fermentation still appears to me however one of the most mysterious of chemical processes; particularly because it operates only gradually, and we cannot conceive why, when the ferment and the sugar are intimately mixed together, they do not act on one another with greater rapidity. We might be tempted to believe, that it is partly owing to a galvanic process, and that it has some analogy to the mutual precipitation of metals.

Theory of Mr. Appert's mode of preserving animal and vegetable substances.

Be this as it may, it seems to me, that we may clearly conceive how animal and vegetable substances are preserved by the process of Mr. Appert. These substances, by their contact with air, readily acquire a disposition to putrefy or ferment: but on exposing them to the heat of boiling water in vessels well closed, the oxigen absorbed produces a new combination, which is no longer capable of exciting fermentation or putrefaction, or which is rendered concrete by heat, in the same manner as albumen*. In fact it is observed, that a juice disposed to ferment, and perfectly clear, becomes turbid at the heat of boiling water, and then is no longer susceptible of fermentation, unless it be placed in contact with oxigen gas. In this case, if it be made to boil as soon as fermentation begins to take place, the fermentation is quickly stopped, and a deposition, of an animal nature, takes place. It may farther be observed, that beer yeast, which has been exposed to the heat of boiling water, likewise loses the property of exciting the fermentation of sugar. Now since must of grapes that has been boiled still retains ferment in solution, which, to produce fermentation, requires only the contact of air; we must conclude, that only the part which has absorbed oxigen, and which is probably in the same state as beer yeast, is capable of coagulating by heat.

Heat destroys the fermenting property of yeast.

This is the idea I have formed of the preservation of animal and vegetable substances: and if, as the experiments I have related seem to prove, oxigen be necessary to the developement of fermentation and putrefaction, it is evident, not only that the heat must be continued long enough

Requisites to

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* Seguin has supposed albumen to be the true principle of fermentation. See Journ. vol. XV, pp. 332, 333. C.

the success of Mr. Appert's process.

Fruits might be preserved in hidrogen or nitrogen gas.

to destroy or render concrete the matter, which has absorbed oxigen, and is calculated to excite fermentation; but also that the vessels in which the substances are to be kept, must be stopped too closely for the air to penetrate them. It is very probable, from this theory, that all sorts of fruits may be kept a long time in hidrogen or nitrogen gas, provided they had absorbed no oxigen. We may conclude too, that, if grapes will keep a long time without fermenting, it is because the exterior coat does not admit the entrance of oxigen; not, as Mr. Fabroni has supposed, from an excellent analysis of grapes, because the ferment and saccharine matter are in separate cells. Lastly I consider it as possible, that, if an animal substance, milk for instance, could be obtained without the contact of air, it would keep a long time without alteration.

Fermentation excited by galvanism.

Action of electricity.

From what has been said it might be expected, that fermentation might be excited in the must of grapes obtained without the contact of air, by immersing in it the two wires of a galvanic battery; and this in fact takes place. But an inference deducible from this is, that it is probably by increasing the electric energy of the various substances in contact, that atmospheric electricity so powerfully promotes the acescence of milk, broth, &c.

Matching of casks.

The experiments I have related throw some light on the brimstoning or matching of wines, which has been practised from time immemorial, without any one hitherto attempting to account for it*.

Sulphurous acid best for preventing fermentation.

Acids, particularly the mineral acids, may prevent fermentation by combining with the ferment, or altering its nature: but sulphurous acid acts like the other acids, and besides seizes the oxigen, which the wine may have absorbed, or which remains in the casks. This proves, 1st, that fermentation cannot commence without the assistance of oxigen; and 2dly, that, at equal degrees of acidity, the sulphurous acid prevents fermentation better than any other.

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* This process, which consists in burning in the casks, that are about to be filled with wine, a greater or less number of sulphuretted matches, or pieces of linen dipped in melted brimstone, might be managed much more simply and economically, by preparing concentrated sulphurous acid with a good apparatus, and afterward adding a small quantity of this acid to the wine intended to be brimstoned.

[In our cider counties, where a similar process is performed, it is usual, I believe, to sprinkle some aromatic seeds, as coriander, over the melted brimstone. C.]

Farther experiments intended.

My labours at present are far from complete. I have instituted many experiments, the results of which yet remain to be known, or which require to be revised; and I reserve them for a more extensive disquisition, that will embrace other objects. I confine myself therefore to this abstract, and shall conclude with observing, that very pure sugar, as well as manna, has the property of dissolving the yellow oxide of lead, and of acting afterward on colours in the same manner as the alkalis.

Action of sugar and manna on lead.

IV.

Note on Prussic Acid: by Mr. GAY-LUSSAC*.

Little information of late respecting prussic acid, though both decomposed and composed,

SINCE the discovery of prussic acid by Scheele, and the labours of Messrs. Berthollet and Clouet, nothing very important has been brought forward on the nature of this acid. Though the mobility of its elements has permitted it to be decomposed, and it has even been composed by passing ammoniacal gas over redhot charcoal, it has not yet been obtained perfectly pure, so that we know not under what form it would present itself in this state. I have endeavoured to solve this question, and I shall prove in this note, that prussic acid is not permanently elastic; that it forms a liquid much more volatile than sulphuric ether, since it boils at 26·5° [79·7° F.]; and that, owing to this property, at a temperature from 20° to 26° [68° to 78·8° F.] it dilates considerably the air or gasses with which it is mixed, communicates to them its properties, and then resembles a permanently elastic fluid.

Its form when pure.

Attempt to obtain it so.

Desirous of ascertaining, for the purpose of a particular inquiry, whether prussic acid might be obtained in the gaseous state, I decomposed prussiate of mercury by muriatic acid, as directed by Mr. Proust. After the air contained in the vessels had escaped, and a strong smell of prussic acid was perceivable, I received the gas over mercury. Thus I obtained several jars full of an elastic fluid, inflammable, and with a powerful smell, which appeared to me to be gaseous prussic acid. However, on continuing the process, I perceived, that drops of a peculiar liquid acquired the gaseous state, as soon as they reached the summit of the jar, and depressed the column of mercury considerably. The temperature then was at 20° [68° F.]; and the next morning, the temperature being only 12° [53·6° F.], I observed, that the bulk of the gas I had obtained was greatly diminished, and that a liquid was deposited in the jars, where there was none before. I had then no longer any doubt, that the prussic acid was a very volatile liquid; and after several trials, which I shall pass over, I succeeded in procuring it readily in the following manner.

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* Ann. de Chim. vol. LXXVII, p. 128. Read to the Institute Feb. 1811.

Method of procuring it.

I took a tubulated retort, into which I put prussiate of mercury; and to the neck of the retort I adapted a curved tube, one end of which I inserted into a small two-necked phial, containing a mixture of chalk and muriate of lime; the chalk being intended to saturate the muriatic acid, that might escape from the retort, and the muriate of lime to retain the water. From this phial another tube proceeded to a second, containing also muriate of lime; and from this issued a third tube, terminating in a third phial with a ground stopple, intended to receive the prussic acid. The apparatus being thus arranged, and all the phials surrounded with a cooling mixture of two parts ice and one salt, I poured some slightly fuming muriatic acid into the retort, and applied to it a gentle heat. The prussiate of mercury soon dissolved, and the liquor appeared to boil. In fact vapours were evolved, which partly condensed in the neck of the retort, forming streaks like alcohol. The process was stopped the moment water began to rise; though more prussic acid might still be obtained: but it is better to separate the first product, and afterwards resume the process.

Rectification of the acid.

All the prussic acid commonly condenses in the first phial. If no water pass over, the muriate of lime remains solid, though immersed in the prussic acid. If, on the contrary, a certain quantity of water have passed over, two very distinct strata of liquid will be obtained; the lower, an aqueous solution of muriate of lime; the upper, prussic acid. This acid is commonly a little coloured in the first phial. To rectify it, as soon as you think proper to terminate the distillation, take out the tube communicating with the retort; stop the aperture by which it entered the phial; and, after removing the frigorific mixture, that surrounded the latter, heat it very gently, either by means of a water-bath or charcoal, so as not to raise the temperature above 30° or 35° [86° or 95° F.]. When this distillation is finished, take away the first phial; and, after the prussic acid has remained a few hours in contact with the muriate of lime in the second phial, pass it over into the third by means of a gentle heat. The rectification is then finished.

Properties of pure prussic acid.

Prussic acid thus obtained is a colourless liquid, as clear as water. Its taste, at first cool, soon becomes acrid and irritating. Though rectified several times over chalk, it faintly reddens litmus paper; but the blue colour returns, as the acid evaporates. Its density at 7° [44·6° F.] is 0·70583. Its volatility is very great, for it boils at 26·5° [79·7° F.]: at 10° [50° F.] it supports a column of mercury of 0·38 met. [14·95 inches]; and at 20° [68° F.] it quintuples the bulk of the air or gasses, with which it is mingled. This property renders the employ of the apparatus I have described indispensable; for, if it were poured from one vessel to another in the open air, a very large quantity would be lost. This property also explains why it has been said by some chemists, that prussic acid may be obtained in the state of a permanently elastic fluid.

Its great volatility.

Freezing point.

Prussic acid exposed to a frigorific mixture of two parts ice and one salt constantly congeals, and frequently assumes a regular figure. I have sometimes seen crystals of this acid resembling those of fibrous nitrate of ammonia. It remains solid at a temperature of —15° [5° F.], but above this point it liquefies.

Singular phenomenon.

The great volatility of this acid, and its congelation at —15° [5° F.], occasion it to exhibit a remarkable phenomenon. If a single drop be exposed to the air at the extremity of a glass tube, or, which is still better, on a piece of paper, it instantly freezes. This congelation, produced by the evaporation of the prussic acid itself, is, I believe, the only one of its kind; for, among all the very volatile liquids, there is not one that freezes at a temperature so little remote from that of melting ice.

I have studied the chemical properties of prussic acid, prepared as I have just mentioned, and shall make known the principal in another paper.

V.

Abstract of a Paper on Triple Salts: by Mr. GAY LUSSAC*.

THE object of this paper is to show: 1, that, in triple salts the acid is commonly divided between the bases in two equal proportions. This is the case in the triple tartrates and oxalates; in the ammoniaco-magnesian sulphate; in the triple sulphate of zinc and ammonia; &c.

The acid of trisules commonly divided equally between the bases.

2. That in a triple compound the elements united two and two form possible binary compounds. For example, the nitrate of ammonia, which is composed of oxigen, nitrogen, and hidrogen, when decomposed by fire yields water, and gaseous oxide of nitrogen: while, on the other hand, this salt is the result of two binary compounds, nitric acid and ammonia.

Elements of triple compounds would form binary ones;

3. That the vegetable and animal substances, which are composed of three or four different matters, also give rise to binary compounds, that are possible, or generally known.

as well as those of higher compounds.

4. That we may form an idea of the different nature of several substances containing the same elements and in the same proportions; if we admit, that the binary products of the elements combine in different ways with each other, or merely with one of the elements.

The same elements in the same proportions may form different compounds.

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* Ann. de Chim. vol. LXXVII, p. 185. From a paper read to the Soc. of Arcueil, February, 1811.

5. That we may imagine so many more compounds containing the same elements in the same quantities, as we can conceive the binary combinations, formed by the elements of these compounds, to be more numerous.

Neutral compounds indicate the capacity of combustibles for oxigen.

6. That salts, or other compounds, being neutral, though formed of an acid containing an excess of oxigen, and a base that is still combustible; we may admit, that the base saturates the excess of oxigen of the acid; and that hence results a point of saturation, well adapted to determine the capacity of combustibles for oxigen. For instance, the neutral nitrate of ammonia, being decomposed by heat, yields as products water, which is neutral, and gaseous oxide of nitrogen, which must be neutral also.

Density of nitrous acid gas.

7. That nitrous gas and oxigen gas, in combining to produce nitrous acid gas, experience an apparent condensation of bulk, which is precisely half the total bulk of the two gasses, whence it follows, that the density of nitrous acid gas is 2·10633, that of atmospheric air being 1.

VI.

Analysis of large-leaved Tobacco, Nicotiana tabacum latifolia and angustifolia: by Mr. VAUQUELIN*.

Tobacco probably contains a peculiar principle.

THOUGH there can be no doubt, that the various methods employed for preparing tobacco modify, each in its own way, some of the principles contained in this plant, yet the changes experienced by these principles cannot entirely destroy their peculiar properties; otherwise, it is evident, that tobacco might be made from a great number of herbaceous plants, which is not the case. Reason therefore leads us to conclude, that there exists in the nicotiana at least one substance, not to be found in the other plants, from which attempts have been made to manufacture tobacco in vain.

Analysis undertaken.

These considerations have led us to undertake a careful chemical analysis of the different species of nicotiana employed in manufacturing tobacco, as well as of the tobacco of different manufactories, both French and foreign.

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* Ann. de Chim., vol. LXXI, p. 139.

Advantages to be expected from it.

We were of opinion, on entering upon this inquiry, that some advantages might result from it to the manufacturer with respect to the preparation of tobacco; or that at least the theory of chemistry might derive from it some principle, by means of which it could give a satisfactory explanation of the changes, that might take place in the matters entering into the composition of tobacco.

Persons engaged in it.

I ought here to mention, that I have been assisted in this long and laborious research by Mr. Robiquet, a very well informed young apothecary of Paris, and Mr. Warden, American consul, who devotes the leisure moments afforded him by his office to the practice of chemistry.

Process.

After having bruised the leaves of nicotiana latifolia in a marble mortar, we wrapped them in a linen cloth, and subjected them to the action of the press. To separate all the soluble matter they might contain, this operation was repeated three times, with the addition of a little water.

Though the cloth was of a pretty close texture, the juice retained a large quantity of green matter in suspension, which was separated by filtration through blotting paper. The green matter that remained on the filter, was washed and set apart, and will be noticed hereafter.

Examination of the filtered Juice.

The filtered juice examined.

1. This juice strongly reddened litmus paper, a proof that it contained a free acid.

2. Oxalate of ammonia, by the copious precipitate it formed, demonstrated the presence of lime, and consequently of some calcareous salt.

3. Nitrate of silver threw down a copious precipitate, which was not wholly dissolved by nitric acid; whence we may infer, that it was partly formed by a muriate.

4. The infusion of galls, and the mineral acids, indicated by the tolerably bulky brown precipitates they occasioned, the existence of some animal matter, particularly of albumen.

5. Heat raised to 80° of R. [212° F.] confirmed this by occasioning a copious coagulation.

6. Acetate of lead formed a very copious grayish precipitate, which dissolved in great part in distilled vinegar.

Examination for malic acid.

The effect of acetate of lead on this juice inducing us to suspect the presence of malic acid, we precipitated by means of acetate of lead a pretty large quantity of the liquor coagulated by heat; and afterward passed through this precipitate washed and diffused in water a stream of sulphuretted hidrogen gas, till there was a slight excess of it.

The intention of this was to reduce the lead to a sulphuret, and thus separate it from the matter with which it had been united. To facilitate the precipitation of the sulphuretted lead, we heated the liquor, and filtered.

Animal matter.

The liquor thus filtered was cautiously evaporated to the consistence of a sirup. In this state it had a very acid taste, strongly reddened infusion of litmus, and formed with alcohol and ammonia copious sediments, which, while they indicated the presence of animal matter, proved, that a portion of it had been carried down with the lead in its precipitation.

The acid dissolved in alcohol.

Hoping that the acid contained in this thickened liquor would prove soluble in spirit of wine, and that we might thus separate it from the matter it held in solution, we treated it hot with this menstruum at 40° [sp. gr. 0·817]. In fact, as soon as the mixture of these two liquors took place, a copious coagulation was produced, and the alcohol became coloured, first yellowish, then brown red, and was found to be acid.

The matter not dissolved by the spirit of wine was whitish, partly dissolved in water, and its solution was precipitated by the acetate of lead like the acid itself.

Oxalate of ammonia occasioned in it a precipitate, and this substance, placed on burning charcoal, left a residuum of carbonate of lime. Lastly, we found, that this substance was formed in great part of malate of lime, which retained some portions of vegeto-animal matter.

The animal matter strongly attracted by the acid.

The greater part of this vegeto-animal matter, which we had endeavoured to separate by means of alcohol, having dissolved in it by the help of the acid, we saturated the latter with ammonia, which threw down a considerable flocculent sediment, the properties of which were perfectly similar to those of animal matters. Notwithstanding this saturation, nutgalls still produced a very evident precipitate in the liquor; whence we perceive there exists a very great affinity between the acid and this animalized principle.

The acid was the malic.

This acid, purified as much as possible, exhibited all the characters of malic acid; that is, it imparted considerable consistency to water by evaporation, did not crystallize, gave with acetate of lead a precipitate soluble in distilled vinegar, swelled up in the fire giving out a smell of burned sugar, and was converted into oxalic acid by the nitric acid.

Malate of lime separated,

Thus the acetate of lead had thrown down at once malic acid, a great deal of coloured vegeto-animal matter, and a little malate of lime. The last appeared to have been carried down in combination with the malate of lead, and to have been redissolved by the malic acid, as fast as it was separated by the sulphuretted hidrogen.

In several experiments, in which we thus precipitated tobacco juice by an excess of acetate of lead, we in like manner found again malate of lime in the malic acid.

which exists in the plant largely.

A large quantity of malate of lime therefore exists in tobacco, which may be obtained directly by evaporating the juice of the plant to two thirds.

The juice deprived of acid distilled.

As soon as we had completely ascertained the nature of the acid, we returned to the juice of tobacco into which we had poured an excess of acetate of lead, to treat it also with sulphuretted hidrogen. We had obtained a very transparent liquid, of a lemon colour, which retained the exact smell and all the acrimony of the unaltered juice. Suspecting, that this taste depended on a volatile oil, we distilled the liquor, and obtained a product, that had a slight herbaceous smell, and but little taste.

Matter left in the retort.

The concentrated portion, that remained in the retort, emitted, on the addition of a little potash or ammonia, a strong smell, which was so pungent, that, if snuffed up with a little force, it occasioned sneezing and tears.

Distilled with the addition of potash.

We repeated the experiment by adding potash to a more considerable quantity of matter, and distilled, after having diluted it with a lit-a little water. The new product, which we obtained in this operation, had the smell of tobacco smoke, was extremely acrid, and produced a similar sensation to that occasioned by a pinch of snuff taken with such force as to get into the throat.

New product.

As this product was alkaline, we suspected, that this principle, whatever it might be, was rendered volatile only by means of ammonia, arising from the decomposition of an ammoniacal salt in the tobacco; since, when the liquor contained an excess of acid, we did not obtain the same result. However, in a similar process, conducted it is true with dry tobacco, we obtained a product, the smell and taste of which were at least equally striking, though the liquor that yielded it contained a free acid. For the rest, we were never able perfectly to isolate this acrid substance, and even the greater part remained in the retort. Hence it appears, that the malic acid diminishes the volatility of this acrid principle.

Attempt to obtain it separate.

In order to obtain this principle separately, we evaporated, by a very gentle heat, the liquor it contained, and treated it with alcohol at 40° [0·817], which in fact separated it from the other matters. On afterward evaporating the alcohol, we remarked at the surface of the liquid some traces of a brown oil; and the portion that distilled over became more and more acrid, as the process approached its conclusion. This nearly solid oil, when thrown on burning coals, emitted a thick smoke, and such a strong smell of tobacco, that it was insupportable.

Nitrate of potash.

The alcoholic solution yielded on cooling some nitrate of potash.

The acrid principle.

The acrid principle in question has little smell when dissolved in water; which shows, that it is not very volatile. It appears very difficult to destroy; for, if it be mixed with a pretty large quantity of oximuriatic acid, it still retains all its acrimony, after the acid has evaporated spontaneously.

Probably peculiar to tobacco.

The peculiar degree of volatility and acrid taste of this substance seem to indicate, that it is a principle belonging exclusively to the genus nicotiana; and which is consequently new, since those chemists, who have analysed this plant, have not spoken of it, at least as far as we know.

Not produced artificially.

Hence we may conclude, that this principle, which is found also in prepared tobacco, as we shall show in another paper, is not altered by the different operations it undergoes; and consequently is not produced by any change in the constitution of the materials of the plant.

Substances found in the juice.

The following are the substances we have thus far found in the juice of tobacco:

1, An animal matter:

2, Malate of lime with excess of acid:

3, Nitrate of potash:

4, Muriate of potash:

5, A peculiar acrid principle.

Now we know these different substances contained in the juice of tobacco coagulated and filtered, we shall point out the results of the successive experiments made on the green fecula, on the coagulum obtained during the boiling of the juice, and on the woody residue.

Examination of the green fecula;

The green matter obtained by filtering tobacco juice, being treated with alcohol, left as an insoluble residuum a grayish substance, tolerably compact, yielding on distillation a great deal of carbonate of ammonia, partly crystallized and partly dissolved in water, a thick fetid oil, and a coal of difficult incineration, which yielded a little lime, proceeding no doubt from a portion of decomposed malate. It appears, that this matter is a portion of insoluble vegetable albumen. As to the colouring portion of the fecula dissolved by alcohol, it differed in no respect from the green fecula of vegetables.

of the matter coagulated by boiling;

We have said, that a pretty considerable coagulum was formed on boiling the filtered juice. This coagulum is an albuminous vegeto-animal substance; for it emits the same smell as horn when burning, and yields a great deal of ammonia; but it is remarkable, that it leaves a great deal of lime after it is burned.

To find whence this earth could be derived, we treated a portion of this animal matter with muriatic acid very much diluted. The solution, filtered and saturated with ammonia, yielded by cautious evaporation a granular powder, which was also malate of lime. What had not dissolved in the muriatic acid was pure vegeto-animal matter.

and of the woody residue.

Something remains to be said respecting the woody residuum.

duum. After it had been well washed, we treated it with alcohol, to take up all the green resin it contained; and we afterward subjected it to the action of weak nitric acid, which extracted from it various calcareous salts. At first, on saturating this acid by ammonia, we obtained a flocculent matter, which, when decomposed by sulphuric acid, yielded an acid liquor, that precipitated lime water in large flocks, which oxalic acid does not. However, as we suspected the existence of oxalate of lime in this woody residuum, we cautiously evaporated a portion of the same liquor, and obtained a few crystals of oxalic acid.

Method of separating phosphate from oxalate of lime

Phosphate of lime may be pretty accurately separated from oxalate of the same base, when they are dissolved in nitric acid, by adding ammonia to the solution, so that a slight excess of acid shall remain. The oxalate of lime will be precipitated for the most part in a pulverulent form, while the phosphate of lime remains in solution; and this may be precipitated afterward by a larger quantity of ammonia.

The mother water of these crystals, evaporated to dryness and calcined, yielded us phosphoric acid. We also perceived in the nitric solution traces of calcareous salt, which we separated by evaporation of the liquor, and which we found to be malate of lime.

Matters in the woody residue.

Thus the nitric acid took up malate, phosphate, and oxalate of lime, from the woody residuum.

Lastly the woody matter, after having been treated by these different agents, left, when burned, some ashes, composed chiefly of silex, with a little lime and iron.

Another process for obtaining the acid principle contained in tobacco leaves.

Another mode of obtaining the acid principle.

Instead of precipitating by means of acetate of lead the juice of tobacco coagulated by heat, as we have related above, the juice may be evaporated by a gentle heat, and, when reduced to about one fourth, suffered to cool.* It will then deposite a pretty large quantity of malate of lime in granular crystals, which will become opake by exposure to the air. On boiling down the solution still farther, it will yield fresh quantities of the same salt; and lastly, when it has acquired such a consistence, as will not allow the saline particles to unite, it is to be treated with alcohol, to dissolve the free malic and acetic acids, the acrid matter, and the sal ammoniac; and to separate the animal matter, which the heat could not coagulate on account of the acids, that held it in solution.

The alcohol, containing in solutions the matter just mentioned, is to be evaporated in a retort. It will carry over nothing with it. The liquor remaining in the retort is to be concentrated anew, and treated a second time with highly dephlegmated alcohol, to precipitate some portion of animal matter, that was dissolved in the former operation by means of a little water.

This second portion of alcohol being evaporated in its turn, what remains is to be dissolved in water; the malic and acetic acids are to be saturated accurately with potash; and the whole is to be distilled to dryness, taking care that it does not burn. The water obtained, though clear and colourless, is insupportably acrid; and what is left in the retort still retains the same property: but on redissolving it several times in water, and distilling it, the operator will at length deprive it almost wholly of its acrid taste, and obtain the principle that produces it dissolved in distilled water.

Matter that accompanies it.

We have not yet satisfactorily ascertained the nature of the matter that accompanies it, and which dissolves with it in the alcohol. This matter has a yellowish red colour; and swells up and is converted into a coal in the fire.

If, after this matter has been divested as far as possible of the acrid principle, the residuum be urged with a stronger heat, an oil will be obtained, and muriate of ammonia will sublime. It likewise yields ammonia from the decomposition of the muriate by the potash of the malate and acetate, which the heat decomposes.

From the experiments here related, it follows, that the juice of nicotiana latifolia contains

Contents of tobacco juice.

1, A large quantity of animal matter of an albuminous nature:

2, Malate of lime with excess of acid:

3, Acetic acid:

4, Nitrate and muriate of potash in notable quantity:

5, A red matter soluble both in alcohol and in water, which swells up considerably in the fire, and of which I do not well know the nature:

6, Muriate of ammonia:

7, Lastly, an acrid, volatile, colourless principle, soluble both in water and in alcohol, and which appears to be different from all that are known in the vegetable kingdom. It is this principle, that imparts to prepared tobacco the peculiar character, that renders it easily distinguishable from every other vegetable preparation: this will be proved in a subsequent paper, which we shall give on snuff.

This principle possibly an oil.

It is possible, however, that this principle may be nothing but a very thin oil, which, on this very account, would possess a certain degree of volatility, and the property of dissolving in water and vegetable acids, as common volatile oils do: for on treating dry prepared leaf tobacco directly with alcohol, we obtained, independantly of the acrid principle, a brown oil, that had nearly a similar taste.

It may be conceived, that this matter existed originally in the plant in the state of volatile oil; and that it has been thickened, and in some measure resinified, by the progress of vegetation and desiccation.

It might be supposed too, with equal probability, that the thick oil, of which we have just spoken, is a part of the green resin, that owes its acrid taste to a portion of the volatile principle, which has combined with it. At least there is no doubt, that prepared tobacco owes the greater part of its distinguishing properties to the acrid principle and the oil that exist in the leaf of the nicotiana, for these two substances produce the same sensations in the mouth and in the nose as tobacco itself.

Smoking tobacco,

In smoking tobacco these sensations are modified by the empyreumatic oil, pyroligneous acid, and ammonia, that are formed during combustion; yet we still distinguish very sensibly those arising from the substances in question.

rendered milder by water.

By passing the tobacco smoke through water, as is done in certain countries, the smell and taste of these two peculiar substances are rendered more mild and agreeable.

In a subsequent paper we shall give an analysis of the dried leaf tobacco, and of snuff, prepared in different countries, in order to make known the effects of art on this plant[*].

VII.

Mineralogical and Chemical Examination of Magnesite, the native Magnesia of Werner: by Messrs. HABERLE *and* BUCHOLZ[†].

Mineralogical description.

THE specimens employed in this description and analysis were sent by counsellor André of Brunn from Hrubschitz, in the lordship of Gromau, in Moravia. Those analysed by Mr. Mitchel, which led Werner to make a separate species of this mineral in his system, were from the same place.

Errours in various writers respecting it.

Various inaccurate oryctognostic descriptions, which have been given in the different elementary works of France and Germany, lead us to wish for a fresh examination of this substance. Thus Reuss and Suckow have said, that magnesite shines when rubbed, that it is light, and that its specific gravity, according to Gerhard, is 0·31: but certainly Gerhard was mistaken, or spoke of some other substance. Guyton too, when he gives its specific gravity at 2·162, is wrong; as the late experiments of Haüy on the same substance show. They who say, that this substance is difficult to break, and that it adheres slightly or not at all to the tongue, have fallen into a still greater errour.

Brongniart.

Brongniart, in his Elementary Treatise on Minerals, describes this substance too slightly, or repeats the errours of others; as for instance when he says, that it is greasy to the touch. In fact he brings under the species magnesite too many minerals, which differ both in their oryctognostic and chemical principles. Thus he unites the meerschaum, or plastic magnesite, with the siliciferous carbonated magnesia of Haüy. But the meerschaum is nothing but a hydrate of silex, which may be made smooth by friction; and this Brongniart gives as one of the characters of magnesite. He is mistaken too when he makes the magnesite of Baudissero and Castellamonte, near Turin, a subvariety of plastic magnesite; for this substance belongs to the magnesite of Mitchel. As to his magnesite of Vallecas, it is a true meerschaum, which we cannot join with Mitchel's magnesite; from which it differs greatly in specific gravity, being of 1·6, and also in tenacity. The plastic magnesite of Salinello of the same author is obviously a variety of steatite, as its analysis shows.

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[*] For an account of the empyreumatic oil of tobacco, and its poisonous effects on the animal economy, by Mr. Brodie, see Journal, vol. XXX, p. 305.

[†] Ann. de Chim. vol. LXXIV, p. 85. Translated from Gehlen's Journal, N° 81 and 82, by Mr. Tassaert.

Haüy.

Haüy appears to be utterly unacquainted with meerschaum, for, in vol. IV, p. 443, of his work, he confounds it with the red Turkish clay, of which pipes are sometimes made; but this is nothing but a bolar earth.

Analyses.

It is pleasing to see how nearly the analyses of magnesite made by Mitchel, Lampadius, Klaproth, and Bucholz, agree. This shows how greatly Wondraschek was deceived, when he ascribes to it 20 per cent of water. We perceive too, that Giobert was mistaken in his analysis of the stone of Baudissero, or that it is not a magnesite. We must also question the accuracy of the analysis of the magnesite of Castellamonte, published by Guyton; which consists, according to him of 46 carbonic acid, 12 water, 26 magnesia, and 14 silex, yet is insoluble in water.

Magnesite described.

Magnesite is found in rounded pieces, sometimes as large as a man's head, and of an earthy aspect.

It is always dense, and formed of earthy particles, dull and of a meagre feel; sometimes with fissures in its interior, but never with rounded cavities; and sometimes, but very rarely, a siliceous nucleus resembling chalcedony is found in the centre.

Specific gravity.

The specific gravity of magnesite, when it has been suffered to imbibe as much water as it will take up, is 2·881; otherwise only 2·456. Haüy has found, that the magnesite of Castellamonte, which contains 14 per cent of silex, was of the specific gravity of 2·781, when thoroughly soaked in water, and of 2·175 previously.

The

Hardness.

The hardness of this stone is less than that of fluate of lime, which scratches it; and greater than that of calcareous carbonate, which it scratches: but it effloresces at the surface, or perhaps it is a combination and absorption of water that takes place, and then it becomes friable and very tender, so as to colour the fingers, and readily yield an earthy powder. In all cases it does not become smooth when rubbed, or change its colour.

Its cohesion is trifling; and it is the more easily friable in proportion as it contains less of alumine and silex.

Fracture.

Its fracture is conchoidal, inclining to even, dull, and a little rough to the touch, but never smooth. The fragments have no determinate form, their angles are more or less acute.

Opake.

It has no transparency, but the thinnest edges are sometimes translucid.

Colour.

Its colour is always of a yellowish gray, or a yellowish dirty white, and black spots or figures are seen proceeding from the surface to the inside. Sometimes too it appears marbled with yellowish gray and blueish gray spots, particularly the siliceous varieties.

Electricity.

When magnesite is rubbed on woollen cloth it acquires the vitreous electricity.

It is not phosphorescent.

Adhesive to the tongue.
Action of water.

It is strongly adhesive to the tongue; and when put into water it absorbs 9 or 10 per cent, and becomes transparent at the edges, but water does not dissolve it. If triturated with water, it does not form an adhesive paste, but a mass easily reducible to powder by drying; and it emits a smell of magnesia, not of alumine: but the impure pieces have a strong argillaceous smell, when thus treated.

Smell.

Effervescent.

It effervesces with concentrated acids, and is dissolved by them, in 24 or 36 hours; only when it contains silex, this is not dissolved.

Infusible.

It is infusible before the blowpipe, or with the strongest fire; but it loses its carbonic acid, contracts in its dimensions, and grows so hard as to scratch glass.

Where found.

According to Mr. André, magnesite is found accompanied with common and earthy talc, as well as with meerschaum, and even magnesian limestone [*bitter kalk*], in a stratum of serpentine in a state of decomposition.

In the superior strata of the decomposed serpentine green chalcedony and opal are found.

The magnesite of Castellamonte, near Turin, is also found in strata of serpentine and talc.

Its uses.

This stone might be used for manufacturing sulphate of magnesia. According to Giobert it is used in Piedmont in the porcelain manufactory. We are informed also, that the meerschaum of Vallecas in Spain is employed in the porcelain manufactory at Madrid.

Stones with which it may be confounded.

Care must be taken, not to confound magnesite with talc or lithomarga; and though Werner says, that there are some varieties of magnesite which are soft to the touch, I believe they are only fragments approaching to steatite, and then they become smooth when rubbed.

Its carbonic acid not absorbed from the air.

We must likewise reject the opinion of Giobert, who thinks, that the magnesite of Baudissero contains no carbonic acid while it remains in the earth, but attracts it subsequently from the air; for hitherto no magnesia combined with water alone has been discovered. If magnesia be found pure, it is always intimately mixed with a large proportion of silex, as is the case in steatite, or speckstein. The analysis published by Giobert too must be considered as faulty. According to him magnesite is composed of

Giobert's analysis.

Magnesia	68
Silex	15·6
Carbonic acid	12
Water	3
and casually it contains sulphate of lime	1·6
	100·2

Analysis.

Chemical analysis.

1st variety described.

A. This first variety has the greatest specific gravity, and the slightest degree of cohesion; emits no argillaceous, but a slight earthy smell; is strongly adhesive to the tongue; has a yellowish white colour, and little figures are observable in it. Externally it is friable, and sometimes soils the hands on touching it.

Action of acids on it.

a. When small bits of this stone are thrown into sulphuric, nitric, or muriatic acid, they dissolve but slowly at the common temperature, and some flocculent matter of a light reddish colour remains. By the assistance of heat the solution proceeds a little more speedily, and is complete. If the bits of stone were of a tolerable size when thrown into the acid, the effervescence took place only at their edges, or in a crack where two edges joined. The most remarkable circumstance was, that the magnesite fell to powder, before it dissolved, which facilitated its solution.

Treated with muriatic acid, with

b. In order to ascertain the quantity of carbonic acid contained in this stone, we took 100 grs of magnesite in fine powder, and threw them into thrice their weight of fuming muriatic acid diluted with an equal quantity of water. The mixture was made in a very tall vessel, previously weighed; which could be heated by placing it on a plate of iron not sufficiently hot to raise any vapours. When the effervescence had ceased, the loss was found to be 52 grs. A few slight flocks, floating in the liquor, disappeared on raising the temperature. To avoid all errour, this experiment was repeated without heat, in a very tall vessel stopped with a perforated cork. In eight hours the effervescence had ceased, and the solution was complete, except a few white flocks, which disappeared during the night. The loss was precisely 52 grs.

and without heat.

Exposed to a red heat.

c. On exposing 100 grs of magnesite to a red heat for an hour in an open crucible, they also lost 52 grs. The residuum, which was of a reddish white, was dissolved in sulphuric acid diluted with water, without the least effervescence: a slight flocculent precipitate of oxide of manganese remaining. This stone therefore contains no water; and hence no doubt arises the tolerable degree of hardness it possesses, as well as the slowness with which it dissolves in acids.

Sulphuric solution evaporated.

d. The solution of experiment *c* was evaporated to dryness, and water poured on the residuum to the depth of two inches. The whole of the salt was redissolved, except a few slight flocks of oxide of manganese, mixed with a little sulphate of lime. On evaporating, and crystallizing at several periods, a little more sulphate of lime was deposited: but the whole, including the oxide of manganese, did not amount to a quarter of a grain. No silex was found in this stone.

Muriatic solution precipitated by ammonia.

e. To the second solution of experiment *b* a little more muriatic acid was added, and it was then supersaturated with ammonia. This threw down a reddish precipitate, which, when well dried, weighed a grain; but by exposure to a red heat it was reduced to half a grain. By a separate experiment this was found to be alumine, containing oxide of iron and manganese. These oxides we separated by dissolving in nitric acid the reddish residuum left by the stone after being heated redhot. The weight amounted to a quarter of a grain. On afterward supersaturating the nitric solution with ammonia, a few flocks of alumine were obtained, which were colourless.

Its component parts.

From the preceding analysis it appears, that this stone is an anhydrous carbonate of magnesia, containing a few atoms of lime, alumine, oxide of iron, and oxide of manganese, which appear to give it its colour. The proportions of this stone, and the pretty considerable hardness it possesses, are surprising. It contains in 100 parts

Magnesia	48
Carbonic acid	52
	100.

Artificial carbonate of magnesia always contains water, though its proportions differ,

From the properties of this stone it appears, that nature possesses peculiar means of producing anhydrous carbonate of magnesia: for, from the experiments I have made and published in Trommsdorff's Journal, the principles of carbonate of magnesia may vary, according to the mode in which it is prepared, but in all cases it contains a large quantity of water. If a solution of sulphate of magnesia be precipitated cold by subcarbonate of soda, we always obtain a carbonate formed of

when precipitated cold,

Magnesia	33
Carbonic acid	32
Water	35
	100.

In this process a large quantity of subcarbonate of soda must be employed, because part of it passes to the state of carbonate; but then the carbonate of magnesia obtained is the lightest possible, and very bulky; unless this be prevented by some mechanical operation, as strong pressure, which however does not alter its component parts.

If, instead of operating with the solutions cold, they be mixed at a boiling heat, 100 parts of the carbonate of magnesia produced will contain

precipitated hot, or

Magnesia	42
Carbonic acid	35
Water	23
	100.

The carbonate of magnesia prepared by passing a stream of carbonic acid gas through water, holding carbonate of magnesia in suspension, or by filtering and leaving to spontaneous evaporation the liquor obtained after precipitating one part of sulphate of magnesia by four parts of subcarbonate of soda, contains in 100 parts

saturated by exposure to carbonic acid.

Magnesia	30
Carbonic acid	30
Water	40
	100.

Here it appears, that the first and third processes approach near each other in the proportions of magnesia and carbonic acid; and, if we leave the water out of the question, they differ but little from the natural stone, though this contains a still larger proportion of carbonic acid.

2d variety.

B. The second variety of magnesite greatly resembles the first in the colour and marbling; but it is harder, and not so heavy. It is also less adhesive to the tongue, and emits a perceptible smell of alumine.

Treated with muriatic acid.

a. 100 grains of this stone in whole pieces, treated as in experiment *A b*, left 51 grs. At first a brisk effervescence took place, after which the stone fell to powder. In twelve hours the whole was dissolved, except a few light flocks, which disappeared by agitation.

Exposed to heat.

b. By an hour's calcination this stone lost 52 grs. The pieces had still a slight cohesion, but might easily be rubbed to powder. Their colour was a reddish white.

Treated with sulphuric acid.

c. After having poured on the residuum of experiment *b* an ounce of water, sulphuric acid was dropped in, till the residuum ceased to dissolve, even with the assistance of heat. A light brown residuum remained, weighing a quarter of a grain, which was oxide of manganese mixed with oxide of iron. The solution being evaporated to dryness, the salt was redissolved in water, and to the last yielded crystals of sulphate of magnesia; only half a grain of sulphate of lime was separated, amounting to a third of a grain of carbonate of lime.

Muriatic solution precipitated by ammonia.

d. The solution of experiment *a* having been treated as in experiment *A e*, yielded a precipitate, that weighed one grain after calcination, and consisted of alumine, containing traces of the oxides of iron and manganese.

This second variety then contains

Its component parts.

Magnesia	46·59
Carbonic acid	51
Alumine	1
Oxides of iron and manganese	0·25
Lime	0·16
Water	1
	100.

It comes very near the first, and differs only in some accidental matters.

3d variety.

C. The variety of magnesite, of which I am now proceeding to give the analysis, is perfectly white, more dense than either of the former, strongly adhesive to the tongue, and has a strong earthy smell. It has neither cavities nor marblings interiorly, but a few specks of silex. Care was taken to analyse only such pieces as contained none of these specks of chalcedony.

Treated with muriatic acid.

a. 100 grains of this magnesite, thrown in pieces into muriatic acid, lost 47 grs, without any heat being applied. A gelatinous residuum remained, which would not dissolve, though an excess of acid was added.

Exposed to a red heat.

b. 100 grs of pieces of this stone, exposed to a red heat for an hour, left 49 grs. The residuum was a little reddish, and dissolved gradually without effervescence in diluted sulphuric acid.

Residuum of the muriatic solution.

c. The solution of experiment *a* was carefully evaporated, and half an ounce of concentrated muriatic acid added with an equal quantity of water. This was boiled and filtered. There remained 4·5 grs of an earth, which readily dissolved in a caustic alkaline lixivium. This, with its insolubility in acids, showed it to be silex.

The solution precipitated by ammonia,

d. The solution separated from the silex in the preceding experiment was supersaturated with ammonia, which rendered it slightly turbid. After the precipitate had subsided, the fluid was poured off. The precipitate, well washed and dried, weighed half a grain, and consisted of alumine, mixed with oxides of iron and manganese.

and decomposed by carbonate of soda.

e. The liquor of experiment *d* was decomposed at a boiling heat by carbonate of soda. The precipitate obtained, after it had been washed and heated redhot, weighed 45·5 grs. It was of a brownish colour. On redissolving it in sulphuric acid, a brown residuum remained, weighing half a grain, and composed of the oxides of iron and manganese. The sulphuric solution yielded sulphate of magnesia, from which a quarter of a grain of sulphate of lime was separated.

Of this variety of magnesite therefore 100 parts contain

Component parts.

Magnesia	45·42
Carbonic acid	47
Silex	4·50
Water	2
Alumine	0·50
Oxides of iron and manganese	0·50
Lime	0·08
	100.

VIII.

METEOROLOGICAL JOURNAL.

1812.	Wind	Pressure.			Temperature.			Evap.	Rain	
		Max.	Min.	Med.	Max.	Min.	Med.			
2d Mo.										
Feb. 5	S E	29·58	29·54	29·560	47	41	44·0	—	0·59	☽
6	N W	29·86	29·54	29·700	47	35	41·0	—	1	
7	W	29·86	29·70	29·780	47	37	42·0	—	0·11	
8	N W	29·96	29·86	29·910	41	36	38·5	·28	0·10	
9	N	29·98	29·95	29·965	43	38	40·5	—		
10	E	29·97	29·87	29·920	45	26	35·5	—		
11	E	29·87	29·60	29·735	48	33	40·5	—	2	
12	S	29·54	29·45	29·495	50	39	44·5	—	4	●
13	W	29·77	29·59	29·680	44	38	41·0	—	0·10	
14	Var.	29·65	29·48	29·565	48	39	43·5	—	0·24	
15	N W	29·69	29·65	29·670	47	41	44·0	·48	5	
16	N W	29·75	29·66	29·705	49	45	47·0	—	0·14	
17	W	29·80	29·46	29·630	50	40	45·0	—	2	
18	N W	30·06	29·80	29·930	46	38	42·0	—		
19	S	30·06	29·97	30·015	53	34	43·5	—		☾
20	S	29·97	29·84	29·905	54	42	48·0	—		
21	S	29·84	29·58	29·710	54	43	48·5	—	0·28	
22	Var.	29·59	29·55	29·570	50	41	45·5	·55	0·32	
23	N W	29·75	29·49	29·620	50	31	40·5	—	1·08	
24	N W	29·95	29·76	29·855	44	34	39·0	—		
25	S	29·40	29·30	29·350	44	32	38·0	—	0·12	
26	Var.	29·76	29·40	29·580	42	30	36·0	—		
27	Var.	29·76	29·70	29·730	50	31	40·5	—	6	○
28	S	29·70	29·65	29·675	47	31	39·0	—	1	
29	E	29·65	29·55	29·600	48	37	42·5	·67	3	
3d Mo.										
March 1	E	29·85	29·65	29·750	48	33	40·5	—		
2	N W	29·97	29·90	29·935	46	25	35·5	—	1	
3	E	29·90	29·86	29·880	44	38	41·0	—	0·22	
4	S W	29·87	29·80	29·835	52	35	43·5	—	3	
5	Var.	30·04	29·75	29·895	47	36	41·5	·30	0·13	
		30·06	29·30	29.738	54	25	41·73	2·28	3·71	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Second Month. 8 Rainy evening. 10. p. m. clear: a fine blush in the evening twilight. 11. Hoar frost. 12. Stormy night. 14. Rainy morning: very stormy day. 16. Wind boisterous all night, with rain. 17. Stormy night. 20. A very fine day: lunar halo at night. 21. Cloudy: a heavy shower of hail about half past 9, p. m.: night stormy. 22. About 9 a. m. came on a great storm of wind and rain, mixed with hail, which continued about an hour: on its ceasing, the clouds dispersed and the wind changed to W. About noon it became again stormy, continuing so at intervals till half past four, when it began to hail with great violence; this was followed by rain, and during the storm there were frequent flashes of lightning and some distant thunder. 23. Cloudy: a large lunar halo: wind high in the night with rain. 24. Very stormy morning: heavy rain about 3 a. m. with the wind very strong from N. W. In an hour after, snow and sleet, with a freezing air: clear evening: the moon bright. 25. Very stormy. 27. 28. Hoar frost. 29. Misty morning.

Third Month. 2. Fine, with occasional clouds. 3. Hoar frost: night rainy. 4. Wet morning. 5. Wet night.

RESULTS.

Winds variable.

Barometer: highest observation 30·06 inches; lowest 29·30 inches;
Mean of the period 29·738 inches.

Thermometer: highest observation 54°; lowest 25°;
Mean of the period 41·73°.

Evaporation 2·28 inches. Rain, &c. 3·71 inches.

For the chief part of the observations in the present period, I am again indebted to my friend John Gibson.

LONDON, L. HOWARD.
Third Month, 23, 1812.

IX.

Notice respecting the Geological Structure of the Vicinity of Dublin; with an Account of some rare Minerals found in Ireland. By WILLIAM FITTON, *M. D. Communicated by* L. HORNER, *Esq. Sec. to the Geological Society**.

Object of the paper.

THE following observations are to be ascribed principally to the late Rev. Walter Stephens. I present them to the Geological Society in their present imperfect form, with the hope that they may attract the attention of mineralogists to the country in the vicinity of Dublin; for they are sufficient to show, that very interesting information may be expected from a correct examination of that district; which from its situation is easy of access, and presents many advantages to the observer. I shall subjoin to a brief statement respecting the geological structure of that country, an account of some minerals of not very common occurrence, recently found in Ireland.

The country described.

The city of Dublin is placed in a flat limestone country, at the distance of about five miles to the northward of a range of mountains, which form the verge of a mountainous district, extending thence for more than thirty miles to the southward. Through this tract there passes, in a south-western direction from the shore on the south side of Dublin bay, a broad body of granite, bounded on its eastern and western sides by incumbent rocks of great variety; the structure and relations of which, as well as of the granite mass, are in many places very distinctly exhibited.

Mines.

Within this mountainous district, distinguished by the interesting and beautiful scenery which it presents, are found the copper mines of Cronebane and Ballymurtagh†; and the lead mines of Glenmalur; the veins of lead ore at Dalkey, and that near the Scalp also belong to it. The stream works commonly called the Gold mine, at the mountain Croghan

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* Transactions of the Geological Society, vol. I, p. 269.

† An account of the metalliferous waters of these mines was published in the Philosophical Transactions so far back as the year 1752, vols. XLVII, and XLVIII.

Kinshela,

Kipshela, are on the southern range of this district and of the county of Wicklow; and gold has been found within it, at another mountain also named Croghan, about seven miles to the northward of that place[*].

Tinstone.

The occurrence of tinstone at the "Gold-mine", where it has been obtained in fragments[†], is a fact which deserves attention; for from the great extent of primitive country in the Wicklow mountains, the probability of finding veins of tinstone there are considerable. Porcelain earth in purity equal to the "China clay" of Cornwall has been found in the lands of Kilranelagh, on the south-western side of this county; and granite in a state of decomposition is found so extensively in other parts of it, that this valuable production may very probably be obtained there in considerable quantity.

Porcelain earth.

Beds of schist upon granite.

The country around the village of Bray, at the distance of ten miles from Dublin, presents within a small space an instructive series of rocks; and the appearances observable at Killiney, first noticed I believe by Dr. Blake of Dublin, particularly deserve attention. Schistose beds are to be seen at that place to a considerable extent reposing upon granite; and the line of junction, which begins here at the seaside, may be traced by the eye for some miles across the country. The regularity of this junction is remarkable on the top of Rochestown hill, adjoining that of Killiney; where ledges of granite, against the foot of which the incumbent rocks incline, present in several places a rectilinear course for many fathoms together. On the shore at the base of Killiney hill, the granite is traversed by numerous veins, many of which themselves consist of granite; and in some instances two granite veins, differing from each other and from the mass in fineness of grain and in proportion of their ingredients, are seen to intersect; one vein often deranging the continuity of the other's direction. The substance of these veins is perfectly continuous with that of the mass through which they run, and the surface of the fracture passes through both without interruption.

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[*] Gold is said to have been found also in the King's River, near the village of Holywood, in the county of Wicklow.

[†] Report by Messrs. Mills and Weaver, Trans. Dublin Society.

The

Sugarloaf mountains.

The conical masses of the Sugar-loaf mountains, with the summits of Brayhead, and Shankhill, resembling them in structure, are composed of quartz; and it may be remarked, that the conical form appears to be in some measure characteristic of mountains composed of this substance; for Mr. Jameson informs me, that he has seen in Lusatia detached conical summits composed of it; and that the well known Paps of Jura, and the conical summits in the mountains separating Caithness from Sutherland, are of the same material; as also is, according to Dr. Berger, the mountain Durnhill, near the town of Portsoy*.

Contact of granite with incumbent rocks.

The actual contact of granite with incumbent rocks has been observed at the following places in the counties of Dublin and Wicklow. On the western side of the granite, in a streamlet joining the Dodder, west of the glen above Billinascorney; at Golden hill, near the granite quarries; and at Kilranelagh: on the eastern side, at Killiney; at the southern extremity of the Scalp; at Tonelagee; near Aghavanagh to the eastward; and at the south-western side of Croghan Kinshela. On the shore of Dublin bay, between Booterstown and Blackrock, a mass of compact limestone is visible within a few fathoms of the granite, but in the interval the rock is concealed.

Rocks of the trap family.

Near Ballinascorney, on the western verge of the granitic mountains nearest to Dublin, rocks of the trap family occur; and thence to the south-westward, along the borders of the counties of Wicklow and Kildare, various intermediate rocks between the granitic tract above mentioned and the limestone of the flat country to the westward will be found. At Arklow rock, on the south-eastern extremity of the county of Wicklow, columnar rocks of the trap family have been observed by Dr. Wollaston and the Rev. Dr. Brinkley.

Varieties of limestone.

The quarries in the more immediate neighbourhood of the city afford many varieties of calcareous productions. The

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Vast thickness of quartz.

* Humboldt states, that in South America, quartz constitutes, exclusively, a mass of more than nine thousand five hundred feet in thickness, which he considers as of a "formation" peculiar to the Andes. He has not mentioned the form of the summits. Tableau Phys. p. 128.

calp of Mr. Kirwan, a variety of limestone, of which an excellent description and analysis have been published by Mr. Knox*, is the prevailing rock. Brown-spar (Jameson) is found in veins at the quarries near Dolphinsbarn; and beds of magnesian limestone were observed by Mr. Stephens in the bed of the river Dodder, at Miltown, and at Classons-bridge, above that place. The petrifactions, which abound in many parts of this limestone country, the calp, and the beds of magnesian limestone afford some of the features which may assist in deciding on the "formation" of Werner, to which it is to be referred; a point of considerable interest, from the great extent which the limestone occupies in the counties of Dublin, Kildare, and Carlow.

Ores. In the peninsule of Howth, which forms the northern side of Dublin bay, gray ore of manganese with brown iron-stone, and brown iron-ore (Museum of Dublin College, Nos. 1067-8, 887.) have been obtained in considerable quantity: and a variety of the earthy black cobalt ore of Werner has been found by Mr. Stephens and Dr. Stokes on the southern side of the hill, forming a crust of a rich blue colour lining the fissures of a rock of slate-clay nearly approaching to whet-slate, (Mus. T. C. D. No. 267): Mr. Tennant has in this substance ascertained the presence of the oxides of cobalt and of manganese; and the discovery of it is important, as it indicates the probability of the existence of other more valuable ores of cobalt in that neighbourhood. Lugnaquilla, which is supposed to be the highest of the Wicklow mountains, is situate to the south-westward of the centre of the mountainous district: I have found it, by the barometer, to be 2455·1 feet above the house of Mr. Greene at Kilranelagh, which is itself considerably elevated above the sea. Heights. Cadeen, a hill detached from the body of the mountains, and forming a striking object from the adjacent flat country, is 1588.9 feet; Baltinglass hill, 631.8 feet; Eadestown, 749·4 feet; Brusselstown, 740·1 feet; Kilranelagh hill, 705·5 feet above the same place†. Of

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* Transactions of the Roy. Irish Acad, vol. VIII, p. 207.

† The first three heights above mentioned are each the mean of three observations, the rest are from single observations, with two excellent barometers. Mr. Greene's house is (by a single observation) 95·08 feet above

Mountains nearest Dublin. Of the mountains nearest to Dublin, one of the highest, Garrycastle, is 1531.7 feet above the level of the road at Ballinteer; and the Three Rock mountain is 1247.9 feet above the same place, the elevation of which is considerable. The highest point of Howth is 567 feet above high-water mark.

Account of Minerals, &c.

Vesuvian. 1. *Vesuvian.—(Idocrase,* Haüy). This substance was observed by Mr. Stephens in specimens found by me at Kilranelagh, where it occurs in irregular crystalline masses, in a rock composed of common garnet of a reddish-brown colour, of quartz for the most part greenish, apparently from the admixture of a lamellar fossil of that colour, and a small quantity of felspar. The crystalline form of the garnet is here often very distinct, but in the specimens hitherto found, that of the Vesuvian is not well exhibited, although some indistinct prisms are to be observed. In general, its particles assume a scapiform aggregation, sometimes approaching to stellular, a form which I have not observed in specimens of this substance from other places; but its fusibility, lustre, colour, and other characters leave no doubt as to its nature.

The blocks of this compound at Kilranelagh were not in their natural place, but their size, their great weight and singular form render it probable, that they were not far removed from it. Garnet rock is described as occurring in beds in primitive mountains, and the country at Kilranelagh is of this description.

It is remarkable, that a compound much resembling that which I have described occurs also in the county of Donegal, whence specimens now in the cabinet of the Dublin Society, and that of Dublin College, No. 30, were obtained. The garnet and vesuvian in these specimens are scarcely to be distinguished from those of Kilranelagh; and, as at that place, are accompanied by quartz, often of a simi-

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above the level of the cross roads at the bridge of Tuckmill, a little village on the river Slaney; the elevation of which above the sea will be very well supplied when the lines of the grand canal shall be extended in this direction, as is now intended.

lar greenish colour; with the addition however of bluish gray granular limestone, and a fibrous substance, not improbably tremolite, mixed with carbonate of lime. I have not seen any felspar in the specimens from Donegal*.

Grenatite.

2. Grenatite. (Staurotite, Haüy). This was detected by Mr. Stephens in crystals in a micaceous compound of which I found a specimen at the Glenmalur lead mines in the county of Wicklow; the crystals are small, but their colour, form, and characteristic crossing are very distinct, and they are infusible before the blowpipe.

Beryl.

3. Beryl. (Var. of Emerald, Haüy). The precious beryl has been found by Mr. Stephens and myself imbedded in granite, near Lough Bray in the county of Wicklow. (Museum of Dublin College, No. 39.) Mr. Weaver has discovered it in blocks of granite, near Cronebane in the same county; and I have found in the Dublin mountains, above Dundrum, specimens probably belonging to the same species.

Andalusite.

4. Andalusite. (Feldspath apyre, Haüy). This has been found by Mr. Stephens and myself, in very distinct specimens, on the north east side of Douce mountain in the county of Wicklow, apparently imbedded in the mica slate of which that mountain is composed, and accompanied by quartz, mica, and a remarkable crystallized substance hereafter to be mentioned. It differs from the andalusite of Spain and of Scotland chiefly by inferior hardness; for although some pieces scratch window-glass, others yield easily to the knife: but the Count de Bournon has observed an equal variation in the hardness of specimens of this substance found by him at Forez†; and I have found that of the Scottish stone to vary very much.

This fossil seems to have been first taken notice of under the name of wurflicher (cubic) felspath by Karsten, who took his description from specimens in the Leskean cabinet now in Dublin‡ (No. 907 *b*, &c.); and from a comparison of these with the specimens from Douce, the identity of Karsten's fossil with andalusite is ascertained. I have not found however, that this claim to the first detection of it has been mentioned by subsequent writers; although his opinion with respect to its affinity to felspar accords with that which Haüy is disposed to adopt. Tableau comparatif, &c. p. 217.

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* Since this paper was written, I have found that this compound from Donegal has been described by Mr. Sowerby. British Mineralogy, August, 1810, p. 133.

† Journal de Physique, XXXIV, p. 453. 1789.

‡ Bergmann's Journal, vol. II, p. 309. Ann. 1788.

To this species is also to be referred a mineral which occurs in great abundance at Killiney in the county of Dublin, first observed there by Dr. Blake, and for some time considered as belonging to a nondescript species. It is most remarkable on the shore at the southern extremity of the cliff under the obelisk hill, where it appears thickly on the surface of beds of mica slate; and it seems to abound also imbedded in the substance of that rock, although less distinctly visible until it has been exposed to decomposition, being less affected by exposure than the rock in which it is contained.

The andalusite, when thus brought to view, appears generally in slender prismatic crystalline pieces rounded at the angles, seldom sharp, promiscuously aggregated, sometimes in a stellular form, and of a grayish-black colour, remarkably contrasted with the lustre and light colour of the micaceous substance in which they appear. But in fresher pieces, the form, colour, cleavage, and other characters of this mineral are distinct; and I have observed an approach to the peculiar appearances, which it presents at this place, in some Spanish specimens, where the crystalline shoots had assumed a scapiform arrangement.

Crystals of indurated talc?

5. The andalusite of Douce mountain is accompanied, as has been mentioned, by a crystallized mineral, the characters of which have much affinity to those of indurated talc; and which is placed under that denomination in the collection of Dublin College (Nos. 405, 6, 7); and a specimen of the same kind, stated to be from Glendalagh in the county of Wicklow, was found in the same collection (No. 404.)

The crystals are rhomboidal prisms, of which the length is in some instances more than twice the breadth, but no acumination is observable. They are easily cut by the knife, faintly translucent, their colour yellowish-gray. Small fragments before the blowpipe appear to swell a little from the separation of the folia on the first application of the heat; they become white, and give with some difficulty a solid white enamel. The specimens to which I have access at present do not enable me to give any detail of the remaining characters.

The connection of this substance with the andalusite of Douce is remarkable; the latter often forming the nucleus of crystals externally of four sides, sometimes filling nearly the whole of the interior; but in other specimens forming little more than an axis, with rounded edges, and of irregular form, from which the folia of the investing talclike substance appear to radiate.

The occurrence of indurated talc in crystals has hitherto been very rare: it is not mentioned by Jameson; and Brochant, though he quotes from Emmerling the rhomboidal prism as one of its forms, expresses doubt as to the correctness of the statement; I therefore do not give that name to the crystals found at Douce, without some uncertainty.

Hollowspar.

6. Hollowspar, Jameson. (Macle, Haüy). Very distinct specimens of this mineral have been found by Mr. Davy, at Aghavanagh in the county of Wicklow; and I have observed it at Baltinglass hill, within a few miles of that place.* I may mention here, that from the appearances of many specimens found in the neighbourhood of Killiney, Mr. Stephens was inclined to suppose, that a connection existed between this singular species and andalusite.

Pitchstone.

7. Pitchstone. This substance is found in a vein traversing granite, in the vicinity of Newry in the county of Down. I am indebted to Mr. Jameson of Edinburgh for much of the following description of its external characters, as it appears there.

Its colour is intermediate between mountain and leek green. It is massive. Fracture small and not very perfect conchoidal.

Internal lustre, resino-vitreous and shining. It exhibits lamellar distinct concretions; the plates are from one fourth to one tenth of an inch in thickness, and are farther divisible into pieces of the rhomboidal form of various angles.

The

Pitchstone.

The surface of the concretions is smooth, and strongly glistening. Slightly translucent on the edges. It scratches window glass, but is easily scratched by quartz. Easily broken. Specific gravity, 2·29. Before the blowpipe without addition it yields a grayish white frothy enamel.

It is in some places porphyritic, containing imbedded minute crystals of feldspar and of quartz.

A letter from a very intelligent observer, who has examined this substance in its native place, states the following particulars respecting its position.

" The vein is first observable in the Townland of Newry, " at the bottom of a bank of granite, about half a mile from " the northern end of the town, on the right of the road " leading to Down Patrick. It crosses the road, and runs " due westward, ending on the side of the great road from " Newry to Belfast. Its length, so far as hitherto observed, " is half a mile.

" The rock, which is covered with mould to the depth of " about a foot, consists of a gray granite. The vein is " about two feet and a half, or two and a quarter in width; " at the places of contact both the granite and pitchstone " are disintegrated, the latter being almost as soft as clay, " but becoming gradually harder, as it approaches the " centre of the vein. The structure of the vein is foliated, " the folia being perpendicular to the horizon, and also to " the walls; and beside these there are seams, that run " longitudinally, parallel to the horizon, and nearly per- " pendicular to the folia."

Although this substance presents some peculiarity, in being divisible into rhomboidal fragments, it approaches in this respect to the pitchstone of Arran, (in lamellar concretions) which holds as it were a middle place between it, and that possessing the more usual characters.

Mr. Jameson has described a vein of pitchstone " running in granite," observed by himself in Arran*; and he states, that " lamellar distinct concretions have been hitherto ob- " served in the pitchstone of that island only.†"

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* Min. of Scottish Isles, 4to, vol. 1, p. 81.

† Jameson's Mineralogy, vol. 1, p. 261.

8. The granular sulphate of barytes, hitherto very rare, has been found, as the Rev. Mr. Hincks of Cork informs me, by Dr. Wood of that city, on the seashore, near Clonakilty, whence a specimen in the Museum of Dublin College, (No. 653) has probably been obtained: it is accompanied by iron pyrites. Granular sulphate of barytes.

9. Wavellite. This remarkable mineral has recently been found in the county of Cork, at Springhill near Tracton Abbey, about ten miles south-eastward from the city. The Rev. Mr. Hincks of the Cork Institution, from whom the specimens that I have seen were obtained, informs me, that it was found at a small distance from the surface, near the base of a hill composed of flinty slate, and that he has seen it adhering to a piece of rock of that description. But it has occurred principally detached in the form of globular nodules, irregularly grouped together, and of various sizes, the longest about an inch in diameter, externally coated with a yellowish brown earthy crust, and within composed of radiating crystalline spiculæ, the characters of which agree very nearly with those of the wavellite from Devonshire, described by Mr. Davy; indeed some of the specimens from the county of Cork are scarcely to be distinguished from some of those obtained at that place. Wavellite.

The most distinct specimen, that I have seen, was a nodule about three fourths of an inch in diameter, in part affected by decomposition, and containing some small spongy cavities. On its external surface indistinct dihedral terminations of the crystalline shoots are discernible; and internally, where it is not decomposed, its lustre is higher and more glassy than is common in the Devonshire fossil. The specific gravity of that part of it, which was very pure and nearly transparent, was 2·34.

The nodules are in some instances decomposed throughout; the spiculæ, having lost their lustre, acquire a dull gray or brownish colour, and become much softer than when unchanged; and Mr. Hincks has seen some of them altogether in the state of clay, apparently from the effect of decomposition.

It would appear that the fluoric acid, of which Mr. Davy has ascertained the presence in the wavellite from Devonshire, exists also in that from Cork: for glass is corroded by heating upon it, in a drop of sulphuric acid, a fragment of the mineral from either of those places.

X.

On the Native Country of the Solanum tuberosum, or Potato. By BENJAMIN SMITH BARTON, *M. D., Mem. of the Am. Phil. Soc. &c. Communicated by* JOHN MASON GOOD, *Esq., F. R. S. Mem. of the Am. Phil. Soc. and F. L. S. of Philadelphia.*

On the native country of the potato.

IN the *Transactions of the Horticultural Society of London*, there is a paper, by sir Joseph Banks, on the native country of the solanum tuberosum, or potato*. I have read this paper, with much satisfaction: but as my opinions on some of the points relative to this question are essentially different from those of the learned and excellent baronet, I have drawn up, without much regard to order, the following memoir, which I beg leave to communicate to the public, through my candid and learned friend, Mr. John Mason Good.

The potato supposed to have been introduced from Virginia:

Sir Joseph Banks thinks it very certain, that the potato, though not exclusively a native of Virginia, was actually imported from that part of the American continent into Europe: and that the seed potato of the English was introduced exclusively perhaps into Britain and Ireland from Virginia.

but it is not a native of North America.

It is my decided opinion, that the potato is not a native of Virginia, or of any other part of the North American continent: that it was not even known, in a *cultivated* state, in any of these more northern latitudes of America: and by consequence, that it was not imported into Europe from these regions of the new world.

I shall

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* An Attempt to ascertain the Time when the Potato (Solanum tuberosum) was first introduced into the United Kingdom, &c., by the Rt. Hon. sir Joseph Banks, bart. K. B., P. R. S. &c. Transactions of the Horticultural Society of London, vol. I, part I, art. ii, London: 1807. [See Journal, vol. XX, p. 1.]

Ground of sir J. Banks's opinion.

I shall here examine the grounds, upon which the opinion of sir Joseph is founded.

"The potato now in use (solanum tuberosum) was brought to England by the colonists sent out by sir Walter Raleigh, under the authority of his patent, granted by queen Elizabeth, 'for discovering and planting new countries, not possessed by christians,' which passed the great seal in 1584. Some of sir Walter's ships sailed in the same year; others, on board of which was Thomas Herriot, afterward known as a mathematician, in 1585; the whole, however, returned, and probably brought with them the potato, on the 27th of July, 1586."

Sir Joseph continues: "This Mr. Thomas Herriot, who was probably sent out to explore the country, and report to his employers the nature and produce of its soil, wrote an account of it, which is printed in De Bry's Collection of Voyages, vol. I. In this account, under the article of roots, p. 17, he describes a plant called openawk. 'These roots,' says he, 'are round, some as large as a walnut, others much larger; they grow in damp soil, many hanging together, as if fixed on ropes; they are good food, either boiled or roasted."

Sir Joseph adds, that "in the Herbal of Gerard, which was published in 1597, there is a figure of the potato, under the name of potato of Virginia; and that this writer tells us, that he received the roots from Virginia, otherwise called Norembega."

The arguments examined.

I shall now examine the different arguments, which sir Joseph has adduced to prove, that the potato was really indigenous in Virginia, in the order in which he has mentioned them.

1. He thinks it probable, that the potato was brought home by sir Walter Raleigh's men, in the month of July, 1586. We have here, however, no proof whatever, that the root in question was brought into Britain at this time, and in particular, that it was brought from Virginia. But sir Joseph assumes it as a fact, that the plant, which Mr. Herriot met with in Virginia, and which he calls *openawk*, is no other than the solanum tuberosum. Nor will it be denied, that the description of the openawk, so far as its roots are concerned,

The openawk brought from Virginia, not the potato.

concerned, applies pretty well to our potato. Yet I think it very certain, that they are very different plants: and of this the learned baronet would himself have been convinced, if he had consulted a figure of the openawk. Whether the plant is figured in De Bry's Collection of Voyages, to which sir Joseph refers, I know not, as I have not an opportunity of consulting that valuable work at present.

Some of the Indians call the potato now hob-be-nac: but this of little weight, as it simply means esculent root.

The opinion, that the openawk of Mr. Herriot is the potato, will, no doubt, at first sight, be thought to derive some weight from this circumstance, that, to this day, some of our Indians call the potato (solanum tuberosum) *hob-be-nac*. But I apprehend that this circumstance is, really, of little consequence in the investigation of the subject: for the same Indians (the Lenni-lennápe, or Delawares) denominate the turnip, which is unquestionably a foreign vegetable, *hob-be-nis*: and others of our Indians call the glycine apios, which is soon to be more particularly mentioned, *hopnis*, or *hapnis*. Moreover, the common name, at this time, in the vicinity of Philadelphia, for the sagittaria sagittifolia, or common arrow-head, the root of which is eaten by the Indians, is *hob-ne*, or *hub-ne*, which is, doubtless, a corruption of the Indian name. It is probable, therefore, that the meaning of all these varieties of a common word is nothing more than "esculent root," or something of the kind: in the same manner as *tuckahoe*, or *tuc-ca-ho*, is the name, in the language of other Indians, for several very different species, and even genera, of plants, the roots of which were eaten frequently in the shape of bread, by the Indians.

The openawk figured by John de Laet.

I have just said, that I have not an opportunity of consulting the work of De Bry. But I regret this circumstance the less, since John De Laet, in his very valuable work the *Novus Orbis*, has given us some copious extracts from the commentaries of Herriot; and among other plants, or parts of plants, which he describes from that very respectable traveller, and mathematician, the Flemish historian has furnished us with a description together with a figure of the openawk.

After speaking of the mays, the papaw (annona triloba, *Linn.*), and some others, De Laet says, "Præter has et alias herbas, etiam radices edules sponte hic (in Virginia) proveniunt; imprimis quas indigenæ *openawk* vocant, rotundæ, juglandi nucis magnitudinis, pares, interdum et multo majores; nascuntur humidis et paludosis locis, plures inter se coherentes et veluti funiculo colligatæ; in aqua coctæ, aut igne tostæ, boni sunt alimenti*."

It differs from the potato

in its leaves

and place of growth.

The openawk, however its roots may seem to resemble that of the potato, must be, I say, a very different plant. A bare reference to the figure in De Laet will be sufficient to show the validity of this assertion. Indeed it has not the most distant resemblance to the potato. Instead of the pinnated leaves of the latter, the openawk has simple ovate leaves. Neither do the places of growth of the two plants agree very well. We are told, that the openawk grows in moist and marshy situations. In such situations who ever thought of planting the potato? and so far as we know any thing of the soil of the latter in Chili, where, if it be not truly indigenous, it has, at least, been most anciently known, it is never found in marshy soil, but in a soil of a very different kind: in the fields and upon the mountains. It is true, however, that De Bry, according to sir Joseph Banks, places the openawk merely in a "damp soil."

De Laet's figures and representations.

It may be asked, why place so much reliance upon the figure of the openawk, in the work of De Laet? I answer, that many of the figures of vegetables, animals, &c., in the *Novus Orbis*, though merely cut upon wood, are far from being inaccurate representations of the objects they are intended for. Linnæus, Willdenow, and other naturalists have not been ashamed to refer to some of De Laet's icons of plants.—See in the *Species Plantarum* polygonum sagittatum.—But, I repeat it, it is sufficient to cast the most superficial glance upon the wooden cut of openawk, to be fully satisfied, that it could never have been intended to represent the solanum tuberosum.

The root of the openawk of Herriot answers to that of glycine apios,

I wish it were as easy to determine, what plant the openawk is, as what it is not. The description of the root answers pretty exactly to that of the glycine apios of Linnæus, a very common plant in many parts of North America, and perhaps no where more common than in Virginia. This fine plant grows in moist situations, in a rich soil, as along the banks of our rivers, &c. It is well known by the name of Indian potato, wild potato, earth nuts, ground nuts, &c. The root is so abundant, and so well tasted, that the plant is worthy of being cultivated; and the more so, as even its seed, or "pease" as they are sometimes called, when boiled, are deemed a delicacy at the table.

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* Novus Orbis, seu Descriptionis Indiæ occidentalis, Libri XVIII. Authore Joanne de Laet Antverp., lib. iii, cap. XXII, p. 90. Ludg. Batav. 1633.

which is a plant well deserving culture:

but De Laet's figure not like it.

But De Laet's openawk is as unlike glycine apios, as it is unlike solanum tuberosum. Indeed, I must dismiss this part of my subject by candidly confessing, that I know not what plant the openawk is. Perhaps, it will be found, in the course of farther inquiry, to be a species of one of the three genera, arum, pothos, or sagittaria.

Other esculent plants mentioned by de Laet.

Beside the openawk, De Laet speaks of, 1. the *okecpenauk*. This is, unquestionably, the vast tuber, mentioned by Clayton (*Flora Virginica*, p. 176), which I call tuber tucca. 2. The *kaistucpenauck*, which has a white root, "ovi gallinæ forma et magnitudine." This I take to be a sagittaria. 3. *Tsinaw*, a climbing plant, of which bread is made, is I suspect, a species of smilax. 4. *Coscushaw*. Of the root of this also the Indians made bread. The plant grows in moist and stagnant places. The recent juice is poisonous and must be expressed before the pulpy and fibrous part can be made into bread. This, certainly, is not solanum tuberosum, but, if I mistake not, a species of arum. I take it to be Captain Smith's *tockawhoughe*. 5. *Habascon*, "a hot root, of the shape and size of the parsnip": perhaps the root of some species of angelica.

Gerard's testimony of little weight.

Habitats of plants frequently given erroneously.

II. I am sorry that I have not an opportunity of consulting the *herbal* of Gerard. But I readily take it for granted, that what this old writer has said relative to the potato is correctly stated by sir Joseph. Allowing this to be the case, the statement is not of very material importance in the present inquiry. Gerard may have meant nothing more, than that the plant was said to have come from Virginia, or Norembega. Every botanist knows how vaguely, or erroneously, the native countries of many vegetables are mentioned, even in some of the best and most classical works on the science. Have not the plants of Canada, New York, and Pennsylvania, been asserted to grow—perhaps exclusively—among the toupinamboes of Brazil? Even Linnæus, speaking of aconitum uncinatum, says "Habitat in Philadelphia." I presume, that he intended to say, Pennsylvania, though I think the plant has not yet been found wild in any part of this state. Hardly any part of botany stands more in need of reform than that which relates to the *habitats* of vegetables. Zoological science, too, in this respect, may be greatly improved and corrected.

Zoology deficient in this respect.

Where was Norembega?

And where was Norembega? I believe the geographers would find some difficulty in determining this point. In De Laet's map of "Nova Anglia, Novum Belgium, et Virginia," we find the word "Norembegua" laid down far to the north of the most northern limits of modern Virginia; somewhere about latitude 45! The historian tells us, that he is at a loss to determine the situation of the celebrated city and river of Norembegua, concerning which many fables have been written. He conjectures, however, that the river of this name is that called by the English "Pennobscot." "*Qui* superioribus annis de hisce regionibus scripserunt, multa fabulati sunt de celebri oppido et flumine *Norumbegua*," &c. Lib. ii, cap. XVIII, p. 55.

The potato not a native of Virginia.

That the potato was not brought from Virginia, and that it is not even a native of that part of North America, will, I think, appear more than probable from this striking circumstance, that not one of the earlier visitors or describers of the country has mentioned this vegetable, in their lists of those which they found, either wild or cultivated, among the Indians. In Mr. Herriot's account there is no reference to any thing like the potato, with the exception of the openawk, and a few other esculent roots, the aboriginal names of which I have already mentioned. And although I may not be able to say confidently what are the precise species of plants called openawk, kaistucpenauck, tsinaw, coscushaw, &c., I think I have been successful in proving, that the solanum tuberosum is neither of them.

Herriot, an acute observer, does not mention the potato.

The silence of Herriot, in regard to the potato, is with me a circumstance of considerable weight. For this writer was no common observer. He seems to have examined, with nice and philosophic attention, the manners, the customs, &c., and to have paid particular attention to the dietetic articles of the Indians. Had he found the solanum tuberosum in Virginia, either as cultivated by these rude people, or as growing wild in their woods, &c., he would not have neglected to give us some information on the subject.

The potato not mentioned as a plant of Virginia by Smith.

Nor is the potato mentioned as one of the *indigenous* plants of Virginia by the famous Captain John Smith, who came into that country in the very first years of the 17th century, and who resided there a long time: certainly long enough to have made himself acquainted with a vegetable of such primary importance to the colony, if it had really existed there; and especially if it had been cultivated by the Indians. It is true, that Smith does make some mention of the potato: and I shall afterward avail myself of what he has said on the subject, as one of the most powerful arguments in support of my opinion, that the potato was entirely unknown in Virginia, when the first English colonists took possession of the country.

The potato not mentioned as a plant of Virginia by any good authority but Mr. Jefferson;

In truth, I do not find, that this vegetable is mentioned as a native of Virginia, or as cultivated in this part of the continent when it was first discovered, by any writer who had enjoyed good opportunity of obtaining precise information on the subject, except by Mr. Jefferson, the learned president of the American Philosophical Society.

and supposed by him to have come from the south.

This gentleman mentions the potato among the vegetables which were found in Virginia when first visited by the English; but (he adds) " it is not said whether of spontaneous growth, or by cultivation only. Most probably they were natives of more southern climates, and handed along the continent from one nation to another of savages*.

His authority for its being known early there not mentioned.

I know not from what source of authority Mr. Jefferson obtained the fact, that the potato was found in Virginia, when first visited by the English. It is not altogether unlikely, that my illustrious friend was misled by the same passage in Herriot, which misled sir Joseph Banks: by the short and imperfect description of the unknown openawk. Mr. Jefferson did not obtain his information from Mr. Beverley, a respectable writer, who published an interesting little work on the history of Virginia early in the sixth century.

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* Notes on the State of Virginia. Original edition, page 68.

Mr. Beverley.

Mr. Beverley could not find the solanum tuberosum in Virginia. He says, indeed, that the native Indians "had originally amongst them Indian corn, pease, beans, potatoes, and tobacco." But he afterward gives a more particular account of their potatoes. "Their potatoes (he says) are either red or white, about as long as a boy's leg, and sometimes as long and big as both the leg and thigh of a young child, and very much resembling it in shape. *I* take these kinds to be the same with those, which are represented in the herbals to be Spanish potatoes. I am sure those called *English* or *Irish* potatoes are nothing like these, either in shape, colour, or taste*".

His account of the Indian potato.

This a variety of the convolvulus batatas.

This is, certainly, an important passage. It almost proves, what I hope to render quite certain in the sequel, that the Indians of Virginia were entirely unacquainted with the solanum tuberosum, when these people were first visited by the Europeans. The long potatoes, which Mr. Beverley mentions, are, certainly, varieties of the convolvulus batatas, well known in the United States by the name of "sweet potato." But it is a fact, that beside this valuable plant, which the Indians of Virginia, &c., have, for a long time, cultivated, these people ate, though I think they did not cultivate, another species of the same genus, the convolvulus panduratus, which is still known in some parts of the United States, by the name of "Indian potato."

The potato not known in Carolina till introduced by Europeans.

Lawson.

Descending farther south into Carolina, we cannot discover, that the Indians of that great tract of country possessed as a native, or cultivated as a foreign plant, the solanum tuberosum, *before* their intercourse with the Europeans. Mr. Lawson, who resided in Carolina, in the very first years of the 18th century, mentions potatoes as some of the "garden roots," that thrive well in Carolina†. He does not mention them in his list of the indigenous plants of the country; and I am led to believe, from his manner of expressing himself, that they were not cultivated by the Indians. It is to be observed, however, that this intelligent traveller mentions the Indian names, among two of the tribes, or nations, of the potato. This, however, from what I have already said, is no manner of proof, that the solanum tuberosum was really an indigenous plant in North Carolina, where Lawson made his principal observations and inquiries. Every one, acquainted with the Indians, has been struck with the quickness of their mental perceptions, and with the rapid ease with which they bestow names, often very significant, upon new objects, especially natural objects, which they have never seen before*.

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* The history of Virginia, in four parts. By a native and inhabitant of the place. Pages 125, 127. London: 1722. The second edition.

† The garden roots, that thrive well in Carolina, are carrots, leeks, parsneps, turneps, potatoes of several delicate sorts, ground-artichokes," &c. A new voyage to Carolina, &c., p. 77. London: 1709. 4to.

Its name in Indian tribes mentioned by him.

The Indians very apt at giving names.

Colonel Hawkins says,

My friend, Colonel Benjamin Hawkins, who resides as public agent from the government of the United States among the Creek Indians; and who is, perhaps, as well acquainted with the history, manners, state of improvement, &c., of these and other southern tribes of savages, as any man in America; assures me, that all the Indians, with whom he is acquainted, agree in considering the solanum tuberosum as a foreign or strange plant in their countries; and that it is only within a very few years, that these people have begun to pay any attention to the cultivation of this plant, which they explicitly say they received from the European Americans, or whites.

that all the Indians ascribe the introduction of the potato to the whites.

Bartram's testimony.

Mr. William Bartram (MS. *penes me*, speaking of the southern Indians of Carolina and Georgia) says, "Their vegetable food consists chiefly of corn (zea), rice, convolvulus batatas, or those nourishing roots usually called sweet or Spanish potatoes; (but in the Creek confederacy, they never plant or eat the solanum tuberosum, or Irish potato, vulgo.")

Other similar arguments

I might, without difficulty, go on to adduce many other proofs, or arguments, similar to those already mentioned, might be adduced.

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* The Tuscaroras and the Woccons, the two Indian tribes mentioned by Lawson, call potatoes, *untone* and *wauk*. These tribes had much communication with the English, at an early period.

ed, all tending to establish my position, that the solanum tuberosum was not found, *either in a wild or in a cultivated state*, in Virginia, or in any of the adjacent countries of North America, by the first discoverers and colonists of these portions of the new world. But it is time, perhaps, to try the question, which we are examining, by another set of arguments.

Smith's testimony.

I have already said, that Captain John Smith does not mention the potato among the "indigenous" plants of Virginia. But this gentleman is not wholly silent on the subject of our plant. On the contrary, his *Historie* contains a memorable passage, which seems to have escaped the notice of sir Joseph Banks, Mr. Jefferson, Mr. Willdenow, baron Humboldt, and all the other writers, who have contended, that the valuable esculent plant, of which we are speaking, was originally found in Virginia: a passage from which it is safe to infer, at least, thus much, *that the potato was not known by the earlier colonists of Virginia to inhabit that country, either in a wild or in a cultivated state.*

His account of the introduction of the potato into Virginia.

Under the head, or date, of 1613, Captain Smith says, that by the return of the ship Elizabeth to Virginia, from England, potatoes were brought into the country. "In this ship were brought the first potato roots, which flourished exceedingly for a time, till by negligence they were almost lost (all but two cast-away roots) that so wonderfully have increased, they are a maine releefe to all the inhabitants." On the margin of the page, we read "A strange increase of potatoes*."

This

Roots mentioned by Smith as indigenous to Virginia.

* The Generall Historie of Virginia, New England, and the Summer Isles: with the names of the Adventurers, Planters, and Governours, from their first beginning An: 1584, to this present 1624, &c. By Captaine John Smith, sometymes Governour in those Countryes, and Admirall of New England. Page 179. London: 1624.—It may not be amiss to take notice, in this place, of some of the roots which Captain Smith mentions as *indigenous* in Virginia. "The chiefe root they (the Indians) have for food is called *tockawhoughe*. It groweth like a flagge in marishes. In one day a salvage will gather sufficient for a weeke. These roots are much of the greatnesse and taste of *potatoes*. They vse to cover a great many of them with oke leaues and

ferne,

This an important fact.

This is, certainly, as I have said, an important passage in the history of the solanum tuberosum, and indeed in the history of the diffusion of esculent vegetables over the world. The potato has most confidently been supposed to be a native of Virginia. From this portion of North America sir Joseph Banks imagines it was brought into Britain, on or about the 27th of July, 1586. Another writer supposes that it was brought from Virginia into Ireland in the year 1623.

Different dates assigned to the importation of the potato.

Not a native of Virginia,

It is now, I think, most satisfactorily shown, that the potato is not one of the indigenous plants of Virginia; and, of course, that it could not have been brought from that country, as early as the year 1586. It is shown, that, so far from being a native of Virginia, this country received its *first* potatoes from Britain, into which they must have been introduced from some other and more southern part of America, by the ship Elizabeth, in the year 1613, about ten years before the period at which these roots are supposed by Mr. Willdenow to have been brought into Ireland from America.

but carried thither from Europe

10 years before some suppose it to have been brought to Ireland from America.

We see too, that, after flourishing very well for a time, the crops were, in a great measure, lost; and that the stock of a very important root was happily preserved by "two cast-away roots," and became, in the course of a very few years, "a maine releefe to all the inhabitants" of a country, the *openawk*, *tockawhoughe*, and similar plants of which, are of small value in comparison of the solanum tuberosum of South America.

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ferne, and then cover all with earth in the manner of a cole pit; over it, on each side, they continue a great fire 24 houres before they dare eat it. Raw it is no better then poyson, and being rosted, except it be tender and the heat abated, or sliced and dried in the sunne, mixed with sorrell and meale or such like, it will prickle and torment the throat extreamely, and yet in summer they vse this ordinarily for bread." *Historie*, &c. pages 26, 27. This is certainly not solanum tuberosum. I take it to be a species of arum, and I think arum virginicum. "They have another roote which they call *wighsacan*." But this Smith mentions as a medicine. He also mentions *pocones* and *musquaspen*. The first of these is used both as a pigment and medicine: the latter merely as a pigment. *Not a word about any thing like solanum tuberosum.*

Sir J. Banks not singular in his opinion of the native country of the potato.

Sir Joseph Banks is not the only late writer, whose correct acquaintance with subjects of natural history entitle their opinions and conjectures to attention, that has assumed it as a fact, that the solanum tuberosum was originally found in Virginia.

Willdenow.

The learned Mr. Willdenow, a botanist of the first order, says—" After America was discovered, many plants were imported, and grew in our climate. The potato was first described by Caspar Bauhin in 1590; and sir Walter Raleigh, in the year 1623, distributed the first which he brought from Virginia, in Ireland, whence all Europe got them*".

Mistakes in this passage, respecting sir Walter Raleigh.

This passage contains some errours, which it may not be amiss to correct.

I. Sir Walter never was in Virginia, though several authors, beside Mr. Willdenow, seem to suppose, that the illustrious Englishman visited, in person, this portion of America.

II. In 1623 Raleigh was not living. Five years before this period (in October 1618), he lost his head upon the scaffold, to the eternal disgrace, if not of his nation, at least of the feeble monarch, who then presided over it.

III. There is, I think, no proof whatever, that Ireland was so exclusively the first European depot of the potato, as Mr. Willdenow supposes it to have been.

Loskiel.

Mr. Loskiel remarks, " Potatoes are originally a North American root, and are said to have been first brought to Europe by sir Walter Raleigh. They are cultivated by some of the Indians†".

Von Humbolt.

On the subject of the potato, Baron de Humboldt has said a great deal; and it is evident, that he considers the history of this root as intimately connected with the history of the Americans.

The potato

" The potato," observes my ingenious friend (with whom I have passed many hours in useful conversation), " presents us with another very curious problem, when we consider it in an historical point of view. It appears certain, that this plant was not known in Mexico before the arrival of the Spaniards. It was cultivated at this epocha in Chili, Peru, Quito, in the kingdom of New Granada, in all the Cordillera of the Andes, from the 40° of south latitude to the 50° of north latitude. It is supposed by botanists, that it grows spontaneously in the mountainous part of Peru. On the other hand, the learned, who have inquired into the introduction of potatoes into Europe, affirm, that the potato was found in Virginia by the first settlers sent there by sir Walter Raleigh, in 1584. Now how can we conceive, that a plant, said to belong originally to the southern hemisphere, was found under cultivation at the foot of the Alleghaney mountains*; while it was unknown in Mexico, and the mountainous and temperate regions of the West Indies? Is it probable, that Peruvian tribes may have penetrated northward to the banks of the Rappahannoc†, in Virginia? or have potatoes first come from north to south, like the nations, who, from the seventh century, have successively appeared on the table-land of Anahuac? In either of these hypotheses, how came the cultivation not to be introduced or preserved in Mexico?"

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* The Principles of Botany, and of Vegetable Philosophy, British translation, p. 390, Edinburgh, 1805.

† History of the Mission of the United Brethren among the Indians in North America. By George Henry Loskiel. Part I, p. 65. London, 1794.

not known in Mexico before the arrival of the Spaniards.

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* Why at the foot of the Alleghaney mountains? admitting, that the potato was really brought from Virginia in 1585 or 1586, it did not come from the *foot* of these North American Andes. No Englishman had penetrated, at this early period, as far as the Alleghaney chain; or even, I believe, as far as the more eastern chains, called the Blue Ridge and North Mountain. The Spaniards, indeed, near half a century earlier than this period, had even *crossed* these mountains in a more southern clime. I allude to the march of F. de Loto's army. And these Spaniards, I may here add, found *no potatoes*. Mr. Humboldt takes the openawk to be the potato. But the openawk was not a mountain plant.

† Why mention the Rappahannoc? Has any one said, that the openawk was found *especially* in the neighbourhood of this river? I have no doubt, however, that Peruvian tribes, that is, Indians specifically, and even varietally, the same, inhabited both the valley of Quito, and the lands which border upon the Rappahannoc, in Virginia; and even upon much more northern streams. But the discussion of this subject belongs to another essay.

"We

No single fact known to connect the history of North and South America.

"We know not a single fact," continues Mr. Humboldt, "by which the history of South America is connected with that of North America. In New Spain, the flux of nations was from north to south. A great analogy of manners and civilization has been thought to be perceived between the Toultecas, driven by a pestilence from the table-land of Anahuac in the middle of the twelfth century, and the Peruvians under the goverument of Manco Capac, It might, no doubt, have happened, that people from Aztlas advanced beyond the isthmus or gulf of Panama; but it is very improbable[*], that by migrations from south to north the productions of Peru, Quito, and New Granada, ever passed to Mexico and Canada".

The openawk of Virginia supposed by Humboldt to be the potato.

"From all these considerations, it follows," says Mr. Humboldt, "that, if the colonists sent out by Raleigh really found potatoes among the Indians of Virginia, we can hardly refuse our assent to the idea, that this plant was originally wild in some country of the northern hemisphere, as it was in Chili. The interesting researches carried on by Messrs. Becmaney, Banks, and Dryander, *prove*, that vessels, which returned from the bay of Albermarle in 1586, first carried potatoes into Ireland; and that Thomas Harriot, more celebrated as a mathematician than as a navigator, described this nutritive root by the name of *openawk*. Gerard in his *Herbal*, published in 1597, calls it Virginian potatale; or *novembega*. The very name by which Harriot describes the potato, seems to prove its Virginian origin. Were the savages to have a word for a foreign plant, and would not Harriot have known the name *papa*[†]?"

Another mistake of Humboldt,

Baron Humboldt seems, upon the whole, to think the solanum tuberosum was really found in Virginia; and that it is the openawk of Herriot. He intimates too, that it was found in a *cultivated* state, in that country. For this suspicion there is no authority. Even the openawk was not cultivated. It is evident, however, that Mr. Humboldt, who is prejudiced by a favourite hypothesis.

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[*] I think it very probable.

[†] Political Essay on the kingdom of New Spain. By Alexander de Humboldt. Vol. II, p. 344—351. English translation. New York, 1811, octavo.

The history of North and South America connected by various facts.

while carrying on his speculations concerning the native country of the potato, lies under the pressure of a favourite theory,—in my opinion a very feeble one,—"that there is not a single fact, by which the history of South America is connected with that of North America". I shall examine this theory in another place. I shall even endeavour to show, by an attention to different species of vegetables, *which were unquestionably found in a cultivated state in the two Americas*, that there is not only a "single" fact, but that there are *many* facts, by which the history of South America is connected with that of North America.

BENJAMIN SMITH BARTON.

Philadelphia,
January the 13th, 1812.

XI.

On the Production of Electrical Excitement by Friction.

By J. D. MAYCOCK, *M. D.*

To W. NICHOLSON, Esq.

SIR,

Law of electrical excitement

THE interesting discoveries of Wilcke, Æpinus, Volta, and other experimenters on the two electricities, together with the result of several experiments by Dr. Davy*, and the facts, which I lately communicated to you†, lead, by a fair induction, to a general law, which may be thus expressed: *The contact and separation of dissimilar bodies operate as a cause of electrical excitement; and the charge, which is assumed, after separation, by one body, is precisely opposite to that, which is acquired by the other.*

It is general

The existence of this law, in relation to a variety of bodies, is fully demonstrated; and, as far as the investigation has proceeded, it appears to affect all in a greater or less degree. I think, therefore, we are warranted, by every princi-

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[*] Philos. Trans. 1807

[†] Journ. vol. XXIX.

ple of rational theory, to receive it as a general and well established law in electrical science. I have already endeavoured to show, that the decomposition of bodies by galvanism depends on the operation of this law; and that the commonly received opinions, respecting the excitement of the galvanic battery, are entirely inconsistent with it. The object of the following pages is to prove, that the excitement of bodies by friction is referrible to the same general law, and is the effect of the contact and separation of dissimilar bodies.

operates in galvanic decomposition, and is the cause of excitement by friction.

In what manner the contact and separation of dissimilar bodies operate as a cause of electrical excitement, I do not pretend to explain; nor am I, at present, anxious to determine, how much of the effect is to be attributed to the contact, how much to the separation: it is sufficient, for the purpose of my argument, to repeat, that no excitement is visible as long as the bodies are in contact; and that, immediately as they are separated, they indicate opposite electricities.

How it causes it not explained.

It must be obvious, that, while we are drawing one body over another, a number of points in the surface of the rubber are first brought into contact with a corresponding set of points in the surface of the body rubbed; that they are then separated from them, and brought into contact with another set of points, and so on, until the one body has passed entirely over the other. Now, at each separation, if the bodies be of different kinds, whether conductors or nonconductors, the general law, we have stated, must operate, and opposite electrical states must be excited in the separated particles. So far, therefore, the excitement by friction, and the excitement by contact and separation, appear to be referrible, in a general manner, to the same principle. We shall now proceed to a more particular consideration of the subject.

Contact and separation the necessary effects of rubbing.

The principal facts, relative to the excitement of bodies by friction, may be expressed in the five following propositions. 1. *To produce excitement by friction, it is essentially necessary, that one or both the bodies employed in the operation be of the class of electrics.* 2. *If two electrics, or an electric and an insulated conductor be employed, the one body will, after the operation, indicate an electricity opposite to that*

Principal facts relative to excitement by friction.

that which is indicated by the other. 3. *The effect of friction performed with one combination of dissimilar bodies is different from that which is produced by any other combination.* 4. *The friction of two bodies, similar in all respects to one another, produces no excitement.* 5. *If the rubber of an electrical machine be insulated, only a very slight charge can be accumulated in the prime conductor; and, under such circumstances, the action of the machine soon ceases altogether.* The agreement of the second, third, and fourth propositions with the general law is too obvious to require being pointed out; and it will not be difficult to show a perfect agreement of the first and last propositions with the same general law, and in this manner to justify its application to the excitement of bodies by friction.

These agree with the general law.

Why one of the bodies must be an electric.

In the first place, let us consider, whence proceeds the necessity of one of the bodies, employed to produce excitement by friction, being an electric. If one conductor be rubbed on another, no evident excitement is produced; for in consequence of the free communication between all parts of a conductor, and between conductors in contact, the charge is removed from each set of particles, immediately as it is excited in them; or, in other words, during the friction of such bodies on one another, opposing powers operate; the contact and separation of dissimilar bodies, tending to produce excitement; and the conducting quality, tending to destroy excitement. If either of the bodies be connected with the Earth, the electrical state of both must be precisely the same as that of the Earth: if they be both insulated, they must possess similar electrical states, as long as they are in contact by the smallest physical points; and consequently there can be no excitement in either; for when excitement is produced by the contact and separation of two insulated dissimilar bodies, one body assumes an electrical state precisely opposite to that, which is acquired by the other. When, therefore, the one conductor has been drawn completely over the other, no more excitement can remain, than what is effected by the separation of the last particles, which had been in contact; a degree inconceivably small, when shared with the whole of the bodies, to which the excited particles belong. As a confirmation of this reasoning, I repeat, that no excitement of the Voltaic plates takes place except the extensive surface of one plate be separated, perpendicularly, from the corresponding surface of the other, so that all points of contact between the two plates be broken at the same instant. If one plate be made to slide over the other, or if, after their separation, they be connected by the smallest points, no excitement is indicated by either.

Excitement of the common electrical machine.

The result is, however, different, if the experiment be performed with two electrics, or with an electric and a conductor; for the charge, which is excited in one set of particles of an electric, is retained by them, and is not shared with the rest of the body, to which they belong; consequently, when any body is drawn over the surface of an electric, it leaves a permanent charge on all parts, with which it comes into contact. As, therefore, the particles of the cylinder or plate of a common electrical machine are separated from the rubber, they acquire a charge, which, as often as they pass near the prime conductor, is partly removed; and from the alternation of these operations, during the revolutions of the electric, a charge is accumulated in the prime conductor, until the whole of this body, if it be insulated, has acquired a degree of excitement, equal to that, which the action of the rubber is capable of giving to each particle of the electric. The prime conductor draws its charge, by degrees, from the excited particles of the plate, or cylinder, as they successively pass near it; but, from its conducting quality, gives it, at once, to any other conductor. It is evident, that no accumulation can take place in the prime conductor, while it is connected with the Earth; and it is equally obvious, that a conducting body, placed near to the prime conductor, will, by removing small charges, as fast as they are produced, effectually prevent its acquiring a high degree of excitement. We may also observe, that the action of an electrical machine becomes more energetic, the longer it is continued; for a repetition of the operations, we have explained, causes a considerable augmentation of temperature, which is favourable to the electrical action of bodies, and adapts the rubber perfectly to the electric, in consequence of which a greater number of points are excited in a given time. The fact, therefore, which is expressed in the first proposition, and which is fully established in practice, is in all respects consistent with the proposed theory.

Necessity of the rubber's communicating with the Earth.

Now, from what has been said, it follows, that, if the rubber do not freely communicate with the Earth, it must become negatively electrified, by the same operation, that gives a positive charge to the prime conductor; and consequently less and less qualified, as the experiment proceeds, to produce a positive charge on the plate or cylinder. We therefore, perceive a sufficient reason, why, if the rubber be insulated, the prime conductor acquires only a low excitement, and the action of the machine ceases altogether in a very short time.

All the phenomena explicable on this principle.

The reasonings I have employed apply particularly to the electrical machine in its present improved state. The principles of my argument may, however, be extended to every experiment, in which excitement is produced by friction, and will, if I have not entirely deceived myself, afford a perfect and satisfactory explanation of the phenomena. I would, therefore, draw my observations on this subject to a conclusion, by stating, that the contact and separation of dissimilar bodies, which have been demonstrated to be a cause of electrical excitement, must operate whenever we employ friction, and that it is capable of producing the principal phenomena, which are excited by friction. This appears to me to form as strong a degree of evidence in favour of a doctrine, as philosophy need require.

A step towards the generalization of electrical phenomena.

The facts, sir, to which I have called your attention, do not immediately point to any bold and extensive views of nature; but they enable us to proceed one step towards a perfect generalization of electrical phenomena; and it is impossible for us to say, to what interesting truths they may ultimately lead. It will, no doubt, be admitted by every one, that an important advantage will have been gained, when we are able to reduce all the means of exciting electricity to one head; as we shall then be better qualified than we are at present, to investigate the relations, which unquestionably prevail, between the first principles of heat, light, magnetism, and electricity.

Excitement of

The excitement of the galvanic battery is a subject yet involved in the deepest obscurity. All the opinions, which have been proposed to account for it, are unavoidably hypothetical, and, indeed, very unsatisfactory: every fact, therefore, which relates to it, deserves attention, although its application may not be clearly perceived. I was induced, some time ago, to try the two following experiments. I filled one of the new porcelain troughs with an acid fluid, so that the metallic plates, and their connecting arc, were completely covered. In this state, a trough of ten pair of plates, 3 inches square, decomposed water very rapidly. Anxious to know how far the division of the trough into cells is at all requisite, I placed the metals, connected by the bar, in a trough without partitions, and filled with the same kind of fluid, but no action ensued. The action which took place in the first experiment appears to be inconsistent with all our theories; and it seems not a little curious, since a communication between the cells is not an impediment to action, that no action was evinced in the second experiment.

the galvanic battery.

Two experiments with it.

It will afford me much pleasure, should these observations call the attention of your readers to the theory of electrical excitement. I trust, that, while we are successfully employing the powers of electricity in chemical analysis, we shall not altogether neglect to investigate the means by which these powers are called forth, and the laws by which their action is regulated. It has, with much injustice, been objected to theoretical pursuits, that they lead to none of the practical advantages, which interest the happiness of society. The remark is indeed true, if applied to particular discoveries; but these are to be considered only as the elements, from which physical science first took its origin, and by which it is daily nourished and supported. Let it never be forgotten, that our most perfect instruments, those which promote no less our comfort than they tend to advance our intellectual improvement, are the invaluable fruits of philophy.

Theoretical pursuits not unimportant.

I am, sir, very respectfully,

Your obedient and obliged servant,

J. D. MAYCOCK.

BATH,
March the 5th, 1812.

XII.

On the Nature of Oximuriatic and Muriatic Acid Gas, in Reply to Mr. MURRAY. *In a Letter from* JOHN DAVY, *Esq.*

To W. NICHOLSON, Esq.

SIR,

Two papers by Mr. Murray on oximuriatic gas.

Why the first was not answered.

Answer to both.

SINCE I last had the honour of addressing you, two papers of Mr. Murray, in opposition to the theory of my brother, Mr. Davy, respecting oximuriatic gas, have appeared in your Journal.—I did not immediately reply to the first, in which I was more particularly concerned, because nothing in that paper required very serious attention; it contained no new facts or arguments in support of the old hypothesis, it consisted merely of observations on a former communication of mine concerning a new gas.—For this reason, and moreover because Mr. Murray promised, that an account should shortly appear of an experimental investigation he had been engaged in, I have hitherto patiently refrained.—The promised communication is now made, and it is now my intention to answer both his papers at the same time.

To the first.

I shall be brief in my remarks on Mr. Murray's former paper. To his incorrect statements I shall oppose merely the results of my experiments. His criticisms on me, I shall, in a great measure, leave to the judgment of the public.

Grounds of the controversy.

That the reader may form some idea of the present state of the controversy, I shall quickly run over its grounds, principally directing the attention to facts.

Mr. Murray's experiment.

Mr. Murray having exposed a mixture of carbonic oxide, hidrogen, and oximuriatic gas, to light; and having found, that no carbonic oxide remained, after the addition of ammoniacal gas; and that the ammoniacal salt formed effervesced with nitric acid; concluded, that the salt was a mixture of carbonate and muriate of ammonia—that the oximuriatic gas had been decomposed; and consequently that Mr. Davy's theory, in which it was considered as a simple substance, was erroneous.

Repeating this experiment, I obtained a similar result: but, as the decomposition of the salt with effervescence was occasioned by *nitric acid*, I did not hastily draw the conclusion, that carbonic acid gas was directly formed without the intervention of water.

Repeated with the same result.

Prosecuting the inquiry I ascertained the existence of an acid gas, consisting of oximuriatic gas and carbonic oxide, which combined with ammonia, and formed a neutral salt, that was not decomposed by *acetic acid*, but with effervescence by *nitric acid*; and which, in all its characters, was as essentially different from a mixture of carbonate and muriate of ammonia, as the new gas itself was from a mixture of the carbonic and muriatic acid gasses. Hence I inferred, that the effervescence Mr. Murray observed was owing to the decomposition of the new ammoniacal salt, formed, I conceived, in his experiment; and that he would have observed no effervescence, had he used the acetic acid instead of the nitric.

A new acid gas, which occasioned the effervescence.

But Mr. Murray was not satisfied with this explanation. He still continued to assert, that the production of carbonic acid in his experiment "was established beyond the possibility of doubt."

The explanation not satisfactory to Mr. Murray.

I grant, that the effervescence is owing to the disengagement of carbonic acid gas. But I deny, that the carbonic acid gas had previously existed in the ammoniacal salt. If this salt was a mixture of carbonate and muriate of ammonia, it would have effervesced with the acetic, as well as with the nitric acid. And I maintain, that the results of my experiment did in no way warrant the liberty, which Mr. Murray has taken with them, of asserting, that they confirmed his statement respecting the direct formation of carbonic acid gas.

The carbonic acid evolved did not previously exist in the salt.

I shall silently pass over the general reasoning advanced by Mr. Murray, in favour of the conclusion he drew from his experiments on the mutual action of the three gasses. I have only to observe, that I have made the experiment, and have given an account of it in a paper sent to the Royal Society on the new gas, and that the result of it was a mixture of the new gas and of muriatic gas. I repeat, that Mr. Murray would not have inferred the formation of carbonic acid

Mr. Murray would not have drawn the inference he did, had he used a different acid.

acid gas, had he used an acid, which did not decompose the new ammoniacal salt.

Answer to Mr. Murray's objection of an inconsistency respecting the new gas;

Mr. Murray has attempted to point out an inconsistency in my account of the new gas. He conceives, that it does not decompose water; and consequently, that its ammoniacal salt cannot, when acted on by an acid. This inconsistency is merely imaginary. The fact is, that the gas, immediately on coming into contact with the water, is decomposed, and converted into the same gasses, that the ammoniacal compound yields when acted on by nitric acid; viz. the carbonic and muriatic. In my first notice of the gas I mentioned its being apparently slightly absorbed by water, only among its most obvious qualities, those which made the first impression on me, and led me to consider it as a new substance.

to his assertion, that he has shown Dr. Davy's opinion to be hypothetical:

As the facts accumulate in opposition to the old hypothesis, Mr. Murray's faith in it seems proportionably to strengthen. He speaks with great confidence of what he conceives he has done. He says, "Mr. Davy's opinion, which was first held out as a genuine theory, admitting of no doubt as being a simple expression of facts, has been shown to be a hypothetical explanation of phenomena. And as an hypothesis not a single proof has been given of its truth." Could assertion supply the place of argument, Mr. Murray certainly would carry his point, and effect all that he conceives he has already done. How he has shown Mr. Davy's theory to be an hypothesis, I confess myself totally at a loss to understand. He has advanced no arguments, that have not been answered; no experiments, the accuracy of which has been admitted; and most of his after papers contain little more than what appeared in his first. What I considered Mr. Davy's theory I still continue to consider it. If it is not an expression of facts, in all its essential parts, to the exclusion of hypothesis, I am greatly mistaken.

and to his remarks on Mr. Davy's style.

Mr. Murray indirectly charges me with a want of candour, calmness, and forbearance, at the commencement of the controversy. Let others decide, whether I deserve this charge, and whether Mr. Murray himself does not, in some measure, merit it. I acknowledge, that I attacked, in my first paper, the old hypothesis with a little warmth, though

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I trust without any arrogance. I did so, because I was perfectly satisfied of the truth of the theory I ventured to defend; and because I opposed opinion merely, and not authorities and persons.—And I hope, if I have been guilty of any impropriety of style, this will in some degree extenuate the fault.

Answer to the last paper.

I shall now proceed, briefly to consider the other paper.

Principles

violated with respect to muriatic and oximuriatic gas.

It is a Baconian principle, not to admit the existence of imaginary things. And it is a principle of modern chemistry, that all bodies, that have not yet been decompounded, are to be considered as simple substances. To introduce unknown bodies into chemistry is as bad, as to adopt occult causes in philosophy. Yet such a licence has been used in respect to muriatic and oximuriatic gas. The former, it has been asserted, is a compound of an unknown something and water; and the latter, a compound of the same unknown basis and oxigen. The presence of water in the one, and of oxigen in the other, instead of being proved, has been taken for granted. Mr. Murray, in his preceding papers, to remove this objection to the old hypothesis, has endeavoured to prove, that oximuriatic gas really contains oxigen; but, since all his experiments for the purpose were found to be incorrect, his attempt was not successful. In his last communication, with the same object in view, he has endeavoured to demonstrate the presence of water in muriatic acid gas, and to obtain water from it by means of a substance not known to contain oxigen.

Mr. Murray's attempts to remove this objection.

His experimentum crucis.

As ammoniacal gas is a substance of this kind, Mr. Murray chose it, as he states, for the subject of an experimentum crucis. He added about 32 cubic inches of alkaline gas to 30 cubic inches of muriatic acid gas over dry mercury. The salt formed was collected in the open air, and introduced into a retort. It had the appearance of being slightly moist; and, when heated, it afforded about 1·3 grain of water: and again transferred to another vessel, and passed in the state of vapour through a heated tube containing charcoal, it yielded more water.

Such is the result of the experimentum crucis, from which it is most confidently concluded, that muriatic acid gas contains water, and that Mr. Davy's theory is unfounded, and not to be maintained except by means of the most unreasonable assumptions.

Its result incorrect.

At first view the result appears improbable, and opposed by several facts; and in a very short time I was convinced by experiments, that it was incorrect. The results, that led me to this conclusion, I shall describe, after I have stated more conclusive evidence.

Dr. Davy repeated the experiment without obtaining water.

The muriate of ammonia, on which Mr. Murray operated, was exposed to the atmosphere in both stages of his experiment previous to distillation. Mr. Davy, my brother, particularly pointed out this circumstance to me; and at the same time informed me, that he had not observed the slightest traces of moisture in making the experiment on a large scale in exhausted vessels; and assured me, that I should not, was not the salt exposed to the atmosphere.

The experiment repeated by Mr. Davy,

In repeating the experiment, which, if accurately made, could not fail of being decisive, I used two mercurial troughs; one for preparing the gasses, the other for combining them in. About 30 cubic inches of each gas were employed. The combination made in a small retort, the capacity of which was about 3 cubic inches, and over well dried mercury; and only one cubic inch of ammoniacal gas was added at a time to one cubic inch of muriatic acid gas, so that all the muriate was collected in the upper and curved part of the retort. Heat almost sufficient to occasion the sublimation of the salt was applied for about ten minutes,

and no water produced:

but no water was produced; agreeably to my brother's result, not even the slightest traces appeared.

but much water appeared when the salt was passed through the air.

I next followed Mr. Murray's example, and collected the salt in the atmosphere, and introduced it into another retort; when, heat being applied, water in no inconsiderable quantity was evolved as he described.

Consequently it was derived from the atmosphere.

Thus we have a demonstration, that the water liberated in Mr. Murray's experiment was not derived from the muriatic acid gas, but from the atmosphere.

Sources of Mr. Murray's mistake.

His errour appears to have arisen partly from too great confidence placed in the accuracy of his experiment; and partly from overlooking, that a light powdery substance like muriate of ammonia, independent of its chemical attraction, absorbs water hygrometrically. Mr. Davy has informed me, that this is the case, and that muriate of ammonia so made absorbs so much, that it even deliquesces.

Muriate of ammonia so made deliquescent.

Mr. Murray's confidence in his result, which is opposed by several facts relative to muriate of ammonia, is to me more surprising than the result itself.

It is well known, that muriatic acid gas condenses its own volume of ammoniacal gas to form muriate of ammonia, which, from trials I have made of its properties, does not appear to differ in any respect from common sal ammoniac. This being the case, if water is liberated on the union of the two gasses, it should, were Mr. Murray's experiment correct, be indicated by an absorption of muriatic acid gas, provided an excess was used.* I have made the experiment, but have not observed the slightest diminution of the gas added in excess.

No water to absorb muriatic gas, if added in excess.

These facts, though mentioned last, first convinced me of the inaccuracy of Mr. Murray's experiment, for they were first ascertained. They confirm the other decisive evidences already brought forward; and, if farther proof was required, I could advance additional circumstances to show, that water is not produced, when the union of muriatic acid gas and ammoniacal gas takes place. As this appears to me to be demonstrated, the necessary consequence is abiding by the experimentum crucis, and renouncing that hypothesis, to which it stands opposed: indeed Mr. Murray allows, that, should the event turn out as it has, such a step must be taken: he allows, if water is not produced, "that it may be concluded, that the water obtained in other combinations of muriatic acid gas has not preexisted in it, but is ready formed;" that Mr. Davy's theory, in short, is correct, and the old doctrine erroneous. Should he not make this acknowledgment, I think he will no longer assert, guided by his own experiment, that Mr. Davy's theory is unfounded, and that it can be maintained only by the most gratuitous assumption; or that to admit it, it is necessary to

Water not produced on the union of ammoniacal and muriatic acid gas.

Necessary consequence of this decisive experiment.

suppose water to exist in ammonia, or to adopt "the hypothesis of unknown quantities of water in gasses."

With great respect, I am, sir,
Your humble servant,
JOHN DAVY.

Edinburgh, Feb. 25, 1812.

XIII.

On the Compensation Pendulums of Lieut. KATER *and Mr.* REID. *In a Letter from a Correspondent.*

To W. NICHOLSON, Esq.

SIR,

Compensation pendulum.

IN your last number I observed the description of a compensation pendulum by Mr. Adam Reid. But this pendulum I conceive to be precisely the same in principle with that invented by Lieutenant Henry Kater, and described in vol. XX, p. 214, of your Journal. The only difference appears to be, that Mr. Reid has used a rod of steel instead of wood, and that his pendulum has no means of adjusting the compensation. It is far from my intention to infer, that Mr. Reid borrowed the idea, but I trouble you with these remarks in justice to the original inventor.

I am, Sir,
Yours with much esteem,
A CORRESPONDENT.

XIV.

A short Account of a new Apple, called the Downton Pippin, in a Letter from THOMAS ANDREW KNIGHT, *Esq. F. R. S. &c. to the Secretary*[*].

DEAR SIR,

New variety of the pippin.

I Sent last autumn a couple of dozens of a new apple, the Downton pippin, for the inspection of the Horticultural

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[*] Trans. Hort. Soc., vol. I, p. 251.

Society, and I hope it will be thought no very humble imitation of the golden pipin, its male parent; being formed by introducing the pollen of this variety into the blossom of an apple provincially known under many names, but most generally by that of the orange pippin, which name however is by no means properly appropriated to it, for the fruit is thickly streaked with red.

How produced. The trees of both varieties were trained to a south wall, and the blossoms of the orange pippin were of course properly prepared for the experiment. The Downton pippin is, in the opinion of a committee of the Herefordshire Agricultural Society, an excellent cider apple, and the hydrometer, as well as the palate, indicates, that its expressed juice holds in solution a large quantity of saccharine matter.

An excellent cider apple.

Its good qualities. The trees of this new variety grow very rapidly, and are so exuberantly* productive, that I am confident the fruit of them may be brought to market at any given price, with more advantage to the grower, than any other good apple cultivated. It ripens a little earlier than the golden pippin, but may be preserved in considerable perfection till March, if not gathered too ripe.

Other new apples. The specimens sent to the Horticultural Society grew in a cold soil, and northern exposure, nor did they afford by any means a favourable sample of this apple†. I hope next autumn to lay before them several other new varities of the apple, obtained by similar means, and which will prove well calculated to supply the place of those, which have been long cultivated, and in which the vital principle is nearly exhausted.

I remain yours,

Downton, Feb. 17, 1809. T. A. KNIGHT.

SCIENTIFIC NEWS.

Wernerian Society.

Porphyry. AT the meeting of this society on the 18th of January, prof. Jameson read a paper on porphyry, in which he described

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[*] Some grafts of the Downton pippin sent to the Botanic garden at Brompton in the spring of 1807, I am informed, have already produced fruit abundantly.

[†] (See Journal, vol. XVIII, pp. 193, 194.)

scribed several species of transition-porphyry as occurring along with gray-wacke, &c., in different parts of Scotland. He also gave a particular account of a floetz-porphyry, which likewise occurs in Scotland, and appears to belong to the old red sandstone formation. The professor conjectured, that this floetz-porphyry may be the mother-stone of the porphyritic felspar lavas, which are found in some countries: and consequently that lavas may occur in rocks of an older date, than those of the newest floetz-trap series.

Floetz-porphyry.

Lavas in rocks of older date than the newest floetz-trap.

At the same meeting Mr. W. E. Leach read a description of two species of shark found in the Scottish seas, illustrative of a proposed subdivision of the genus squalus of Linnæus.

Shark genus.

At the meeting on the 1st of February a communication from Lieut. Col. Imrie was read, containing an account of the district of country in Sterlingshire called the Campsie Hills, illustrated by some interesting geological facts observed by the Colonel on the coast of the Mediterranean. The Campsie Hills consist of trap rocks of great thickness; under which sandstone occurs; and below this lie beds of limestone, with slate-clay, clay iron-stone, and some seams of coal. The trap is in some places distinctly columnar; and in many other places, it shows a tendency to this form. He observed, that these circumstances might give occasion to some geologists to class the trap of the Campsie district with volcanic products, of which however he saw no symptom. He then pointed out, that nature produces these forms both in the moist and in the dry way, and gave examples of both. In the *moist* way, he said, that these forms are seen in greatest perfection in warm climates; and drew his example, in this mode, from the coast of Africa, near the site of ancient Carthage; where a small lake with a deep clay bottom had been accidentally drained by the breaking down of a part of its barrier, and where the clay deposit had split into vertical columns eighteen feet high, and from a foot and a half to three feet in diameter. The example in the *dry* way, he took from the island of Felacuda, one of the most westerly of the Lipari islands. In the lavas of that island, which have taken the columnar form, he mentioned having seen obsidian and pumice, which had been in flow with the lava, and are seen combined in one of its congealed streams.

Geology of the Campsie hills.

Columnar trap.

This structure produced both in the moist and dry way.

The moist exemplified from Africa:

the dry from Felacuda.

Geological Society.

Green waxy substance in alluvial soil.

March the 6th. An additional notice by A. Aikin, Esq. Sec. G. S. respecting a green waxy substance found in the alluvial soil near Stockport was read. The purport of this notice was to mention the discovery of a similar substance at the foot of the hill Meuil Montant near Paris by Mr. Patrin. It there occurs in alluvial sand accompanied by fresh-water shells.

Whin dike in Northumberland.

A communication addressed to the Secretary by the Hon. Henry Grey Bennet, M. P., respecting a whin dike in Northumberland, was read.

Its effects on the strata in contact with it.

The dike here described, is best seen at Beadnel bay where it forms a kind of pier about 27 feet wide and 300 yards long. It rises in a perpendicular position through several beds of stratified rocks, without occasioning any change in their dip or direction. But the qualities of the different strata, where they are in contact with the dike, differ very notably from those exhibited by the same strata at a little distance from the dike. The limestone in particular of both the beds, that are cut through, is harder, more granular and sparry in the vicinity of the dike, and is, farther, incapable of being burnt into good lime.

Native oxide of tin.

Minute crystals and Dr. Wollaston's goniometer preferable for ascertaining the angles.

The reading of Mr. Phillips's paper on the native oxide of tin of Cornwall was continued. Before entering into the crystallographical history of this substance, Mr. P. makes some remarks on the kind of crystals best adapted for goniometrical researches, and states his reason for preferring the more minute crystals to the larger ones, and the reflecting goniometer of Dr. Wollaston to that in common use. He then proceeds to state the means, by which he succeeded in obtaining fractures exhibiting the structure of the crystals, from which it appears, that their primitive form is that of an octaedron composed of two pyramids united by their bases, which are square, and that this is farther divisible through both its diagonals into irregular tetraedrons.

Oxide of tin.

March the 20th. The reading of Mr. Phillips's paper on the native oxide of tin of Cornwall was concluded. After describing the primitive figure of this substance, Mr. P. proceeds to an enumeration and description of those modifications, with their varieties, which have been observed by him, and specimens of which are at present in his cabinet.

Twin crystals.

After describing twelve modifications, the paper concludes with details of those compound crystals usually called macles; of the still more compound ones, which are formed by the junction of two macles; and of the most compound of all, which are macles of macles.

Castle hill, near Newhaven in Sussex.

A description of Castle hill near Newhaven in Sussex, by Hen. Warburton, Esq., Memb. of the Geo. Soc., was read. Castle hill is a small circular elevation, composed of nearly horizontal beds, lying above the chalk in the following order, beginning from the most recent:—1, Sand and rounded flint pebbles. 2, A congeries of oyster shells. 3, A bed of broken bivalve shells, chiefly of the genus *Venus*. 4, A bed of blue clay, enclosing a seam of martial pyrites 3 or 4 inches thick, composed entirely of casts of bivalve and turbinated shells. 5, A bed of indurated marl, the lower part of which is obscurely slaty, and contains between its laminæ leaves apparently of some tree of the willow tribe converted into coal. 6, A seam of coal three or four inches thick. 7, Marl, of a sulphur yellow colour, including large crystals of gypsum. 8, Sand. 9, Chalk.

Accidental sublimation of silex.

A notice respecting an accidental sublimation of silex by Dr. Mac Culloch, Mem. Geo. Soc., was read. A mixture of the oxides of tin and lead was put into an earthen crucible, and covered by another inverted over it: the mass was exposed to a high heat, and on opening the crucibles the empty part of each of them was found lined with capillary shining crystals, which by the usual methods of analysis were proved to be pure silex.

To Correspondents.

I find myself again unfortunately obliged to postpone my answer to A. H. Z. till next month.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXXI.

ARTICLE I.

Observations and Experiments on Vision. By WILLIAM CHARLES WELLS, *M. D. F. R. S.*[*]

Uncommon di
ease of vision

1. I WAS consulted, in the beginning of the year 1809, upon a disease of vision, which, as far as I know, has not hitherto been mentioned by any author. The subject of it was a gentleman about thirty-five years old, very tall, and inclining to be corpulent. About a month before I saw him, he had been attacked with a catarrh, and as this was leaving him, he was seized with a slight stupor, and a feeling of weight in his forehead. He began at the same time to see less distinctly than formerly with his right eye, and to lose the power of moving its upper lid. The pupil of the same eye was now also observed to be much dilated. In a few days the left eye became similarly affected with the right, but in a less degree. Such was the account of the case, which I received from the patient himself, and from the surgeon who attended him. The former added, that previously to his present ailment his sight had always been so good, that he had never used glasses of any kind to improve it. On examining his eyes myself, I could not discover in them any other appearance of disease, than that their pupils, the right particularly, were much too large,

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[*] Phil. Trans. for 1811, p. 378.

and that their size was little affected by the quantity of light which passed through them. At first, I thought that their dilatation was occasioned by a defect of sensibility in the retinas; but I was quickly obliged to abandon this opinion, as the patient assured me, that his sensation of light was as strong, as it had ever been during any former period of his life. I next inquired, whether objects at different distances appeared to him equally distinct. He answered, that he saw distant objects accurately, and in proof told me what the hour was, by a remote public clock; but he added, that the letters of a book seemed to him so confused, that it was with difficulty he could make out the words which they composed. He was now desired to look at a page of a printed book through spectacles with convex glasses. He did so, and found that he could read it with ease. From these circumstances it was very plain, that this gentleman, at the same time that his pupils had become dilated, and his upper eye-lids paralytic, had acquired the sight of an old man, by losing suddenly the command of the muscles by which the eye is enabled to see near objects distinctly; it being known to those, who are conversant with the facts relating to human vision, that the eye in its relaxed state is fitted for distant objects, and that the seeing of near objects accurately is dependent upon muscular exertion.

Another instance.

The disease of which I have spoken is perhaps not extremely rare. For having related the preceding instance of it to Mr. Ware, a fellow of this society, he was kind enough shortly after to send to me a young woman, who appeared to be likewise affected with it. But as I saw her only once, and had not then sufficient time to examine her case minutely, I speak with diffidence concerning its nature.

Effect of the juice of belladonna on the eye.

II. After I had reflected frequently upon these cases, it occurred to me, that, as the juice of the herb belladonna, when applied to the eye, occasions the pupil to dilate considerably, and to become unalterable by light, an effect might at the same time be produced by it upon vision, similar to that which I have just described. I had, indeed, in the course of a few years immediately preceding, applied belladonna several times to my own eyes, without observing

State of the author's eyes.

Power of adapting them to distance lost.

serving any change in my sight, beyond what I referred to the increased size of the pupils; but as I had not looked for any other, I thought it possible, that some additional one might have happened, without my having perceived it. I resolved therefore to make the experiment anew. But to conduct it with precision, it was previously necessary to know, to what extent I possessed the faculty of adapting my eyes to different distances. On this subject I had made many experiments with great care, nearly twenty years before, and had ascertained*, that with my left eye, which was more perfect than the right, I could bring to single points on the retina pencils of rays, which flowed from every distance, greater than that of seven inches from the cornea. In the mean time, however, my eyes had altered considerably, with respect to their seing near objects distinctly, and I had, in consequence, been obliged, not only to use convex glasses, but to change them several times for others of higher power. No dependance therefore being now to be placed on my former experiments, in regard to the present state of my sight, I repeated them, and found, to my great surprise, that the power I once possessed of adapting my eyes to different distances was entirely gone; in other words, that I was now obliged to regard all objects, whether near or remote, in the same refractive state of those organs. I found also, that my eyes, considered as mere optical instruments, were nearly the same as they had been in my youth, and that the convex glasses which I used did very little more than supply, with respect to near objects, the place of a living power which I had lost, without compensating, except in a very small degree, for any alteration in the external shape of the eye, or any change in the configuration of its interior parts. I ascertained, for instance, that to give my left eye the refractive power which it formerly possessed while in its most relaxed state, that by which it was enabled to bring a pencil of parallel rays to a point on the retina, a glass of thirty-six inches focus was fully sufficient; whereas to produce an equal effect upon rays proceeding from a point at the distance of seven inches from my eye, the other extremity of my ancient range of perfect vision, I was now obliged to employ a glass having a focus of only six inches. I regret much, that I had not made such experiments frequently before, as I think it very probable, that I should have found a period in the progress of my vision to its present state, in which my capacity of seeing distant objects was the same as in my youth, and when therefore the whole of my imperfect vision of near objects would have been owing to a loss of the muscular powers of my eye.

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* Essay on Single Vision with two eyes, &c. p. 137.

Experiments on an old eye wanting the crystalline inconclusive.

As there can be no good reason for supposing, that the changes which have occurred in my eyes are different from those, which the eyes of by far the greater number of persons, who are not short-sighted, undergo at the approach of old age, it is evident, that the experiments of Dr. Young* on the eye of Hanson, whom the learned author considered as a very fair subject for such trials, furnish no proof, that the want of the crystalline lens disables a person from having perfect vision at different distances; for as Hanson was sixty three years old, it is highly probable that the results of the experiments would have been exactly the same, if he had still possessed that part of his eye.

Experiments with belladonna.

III. Having discovered, that my own eyes were unfit for the experiments, which I wished to be made with belladonna, I instructed an ingenious young physician, Dr. Cutting, from the island of Barbadoes, and now residing there, in the manner elsewhere described by me †, of ascertaining his range of perfect vision by means of luminous points. This he found, in consequence, to begin, with respect to his left eye, at the distance of six inches, and not to terminate at the distance of eight feet; beyond which he could not see clearly the object, with which he had hitherto made his experiments, the image of the flame of a candle in the bulb of a small thermometer. The flame of a lamp, distant about sixty yards, gave a faint indication of its rays meeting before they fell upon the retina; the rays from a star had very evidently their focus a little before that membrane. He now applied the juice of belladonna to his left eye. Half an hour after, when his pupil was but little dilated, perfect vision commenced at the distance of seven inches; in fifteen minutes more, it began at the distance of three feet and a half. When his pupil had acquired its greatest enlargement, the rays from the image of the flame of a candle, in the bulb of a small thermometer at the distance of eight feet, could not be prevented from converging to a point behind the retina. The rays from lamps still more distant, and from stars, had their focusses at the same time on the retina. This state of vision continued, in its greatest extent, to the following day; and it was not till the ninth day after the application of the belladonna, that he completely recovered the power of adapting his eye to near objects. While his left eye was thus affected, the vision of the right remained unaltered.

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* Phil. Trans. 1801, p. 66: see Journal 4to series, vol. v.

† Essay on Single Vision, &c p. 116.

Dilatation of the pupil not always proportional to the range of vision.

Dr. Cutting remarked, while his left eye was returning to its natural condition, that the diminution of the pupil, and the increase of the range of perfect vision, did not keep regular pace with each other; but that, after his pupil had nearly returned to its former size, his capacity of adapting the eye to different distances was still very limited. As these effects therefore are not inseparably connected, they may occur in others in a different manner from that which he observed. A great degree of dilatation, for example, may take place in the pupil, without a total want of the power to adapt the eye to different distances.

The experiment repeated with the other eye.

Though I could not doubt the accuracy of Dr. Cutting's observations, more especially as the altered state of his eye had lasted a considerable time, and as he had not been prevented by other occupations from attending minutely to the appearances, which were consequent upon it; yet, as he was the first person who had ever applied belladonna to his eye, for the purpose which has been mentioned, and as the results had been remarkable, I requested him to repeat the experiment with his other eye. He complied with my desire, and found, that the appearances which followed were similar to those, which had been produced by the application of belladonna to his left eye.

The belladonna seemed to have done something more than suspend the adapting power.

It will, perhaps, be thought extraordinary, that Dr. Cutting's eye in its relaxed state, before the application of the belladonna, brought parallel rays to a focus anterior to the retina; but that similar rays met in a point upon the retina, while the eye was under the full influence of that substance; as it may hence seem, that the belladonna had done more than merely suspend the exercise of the power, by which the eye is fitted to see near objects distinctly. An observation drawn from the former state of my own sight will, I expect, make this matter plain.

Different appearances of stars on the eye.

When I enjoyed the faculty of adapting my eyes to objects at different distances, the rays of a star, which was viewed attentively by me, always met in a point a little before the retina*; whence I at first concluded, that my eye was unfit for accurate vision by parallel rays. But I afterward found, that if I looked at a star carelessly, its rays had then their concourse on the retina. In the former case, from long habit, originating in my having chiefly viewed near objects with attention, some small exertion was made for the accurate view of a distant object, though none was requisite; in the latter, all demand for exertion ceasing, my eye fell into the most relaxed condition, that by which it was fitted for parallel rays. Dr. Cutting's eye seems to have been similar to what my own once was, in regard to such rays; but as he had not acquired the faculty of viewing a distant object, without making some exertion, the rays from a star crossed one another in his eye before they came to the retina. The capacity, however, of making any exertion was taken away by the belladonna, and pencils of parallel rays were, in consequence, brought to points upon that membrane.

Effects of age on short sight.

IV. Being now in possession of a new instrument, I next attempted to gain, by means of it, some illustration of the changes, which the vision of short-sighted persons undergoes from age.

General mistake respecting it.

It has been very generally, if not universally, asserted by systematic writers upon vision, that the short-sighted are rendered by age fitter for seeing distant objects than they were in their youth. But this opinion appears to me unfounded in fact, and to rest altogether upon a false analogy. If those who possess ordinary vision, when young, become from the flatness of the cornea, or other changes in the mere structure of the eye, long-sighted as they approach to old age, it follows, that the short-sighted must, from similar changes, become better fitted to see distant objects. Such appears to have been their reasoning. But the course pursued by nature seems very different from that which they have assigned to her. For of four short-sighted persons of my acquaintance, the ages of whom are between fifty-four and sixty years, and into the state of whose vision I have inquired particularly, two have not observed that their vision has changed since they were young, and two have lately become, in respect to distant objects, more short-sighted than they were formerly. As the manner, in which this change has occurred, is unnoticed, I believe, by any preceding author, I shall here relate the more remarkable of the two cases.

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* Essay on Single Vision, &c. p. 138.

Short sight for distant objects increased.

A gentleman, who is a fellow of this society, became short-sighted in early life; and as his profession obliged him to attend very much to minute visible objects, he for many years wore spectacles with concave glasses almost constantly, by the aid of which he saw as distinctly, and at as great a variety of distances, as those who enjoy the most perfect vision. At the age of fifty, however, he began to observe, that distant objects, though viewed through his glasses, appeared indistinct, and he was hence led to fear, that his eyes were affected with some disease. But happening one day to take up, in an optician's shop, a single concave glass, and to hold it before one of his eyes, while his spectacles were on, he found to his great joy, that he had regained distinct vision of distant objects. With regard to such objects, therefore, he had lately become shorter sighted than he had formerly been. But along with this change, another occurred of a directly opposite kind. For when he wished to examine a minute object attentively, such as he used to see accurately by means of his spectacles, he now found it necessary to lay them aside, and to employ his naked eye. He had become, therefore, in respect to near

Short-sighted persons become more so for remote objects, less so for near ones.

near objects longer-sighted. The power, consequently, in this gentleman, to adapt the eye to different distances, is either totally lost or much diminished; but the point, or small space to which his perfect vision is now confined, instead of being the most remote to which he could formerly accommodate his eyes, as is commonly the case with the ordinarily sighted when they are becoming old, is now placed *between* the two extremes of his former range of accurate vision. The eyes of the other short-sighted person, a physician of considerable learning, whose vision has been altered by age, have been affected in a similar manner, but not in so great a degree.

Similar instance.

Range of perfect vision lessened both ways by age.

Experiments with belladonna on a short-sighted person.

As the only change, which had occurred from age in the sight of such of my acquaintance as were considerably myopic, was a lessening, on both sides, of their range of perfect vision, I conceived, that this might be the ordinary procedure of nature in such cases, and that it might be imitated, in a young short-sighted person, by the application of belladonna to his eyes. I have hitherto not been able to obtain permission to make the experiment on any young person, who is very short-sighted. Two gentlemen, however, who are somewhat short-sighted, have readily submitted to it; one of them, Mr. Blundell, a diligent and ingenious student of medicine; the other, Mr. Patrick, a well educated young surgeon in London. The first experiment was on Mr. Blundell, and the apparent result was, that the range of his accurate vision was considerably diminished at both ends, but not annihilated. Mr. Blundell, however, afterward informed me, that he repeated the experiment with more care in the country, and found, that in one eye the nearest point of perfect vision was moved forward about two thirds of the whole range, and in the other about one third; but that, with respect to both eyes, the most remote points of the ranges were unchanged. He added, that while one eye was under the influence of the belladonna, the other became shorter-sighted than it had been before; but the difference was not so great, as to induce me to place entire confidence in the justness of his observation. I think it right to mention here, that from mistake I applied only two thirds of the ordinary quantity of belladonna to his eye, in the first experiment; and that he probably, in consequence of my example, applied no more when he made the second; as this might have been the reason, that during both experiments he retained, in part, the capacity of adapting his eyes to different distances.

Another experiment

The experiment on Mr. Patrick was conducted by myself, after he had been frequently exercised in observing the extent of his perfect vision. The results were similar to those which had been remarked by Dr. Cutting. The power of altering the adaptation of his eye, according to the distance of the objects viewed, was for some time entirely lost, and his sight became accurately fitted for such only, as were placed at the farther extremity of his former range of perfect vision. While one eye was under the influence of the belladonna, the vision of the other was unaffected.

The juice affected only the eye applied to.

Did not produce the effects of age.

From these experiments it seems probable, that belladonna will in no case produce the same effect upon a young short-sighted person, that age has produced in the two instances of which I have spoken. I expect, however, to have an opportunity of repeating the experiment on two persons, who are very considerably short-sighted; and I shall take the liberty of communicating the result to the Royal Society, together with some observations I have already made, and others which I hope to make, respecting those persons, who seem to retain to extreme old age the power of seeing perfectly, as far as the accommodating power of the eye is concerned, both distant and near objects; and of others, who, after being without this power for many years, appear to regain it at a similar period of life. Probably the making known my intention may facilitate its accomplishment, by inducing other Fellows of the Society to furnish me with opportunities of increasing my knowledge of these subjects. In the mean time, I shall offer a few words upon two other topics in vision, which seem to derive illustration from my experiments with belladonna.

Farther experiments to be made.

Power of the eye retained in old age,

or recovered.

V. 1. Not only do the pupils move together, when both eyes are in a healthy state, but the pupil of one eye affected with gutta serena moves with the pupil of the other, as long as this remains sound. These facts are generally, but in my opinion erroneously, attributed to the immediate sympathy

Moving of both pupils together,

not owing to immediate sympathy.

pathy between the pupils. For when the pupil of one eye becomes dilated from the application of belladonna, the pupil of the other, so far from dilating, becomes smaller. It follows, therefore, that the size of the pupil is dependant, not only on the impression of light on the retina of its own eye, but on that also which is made on the retina of the other; and that the moving of the two together, which for the most part takes place, is only an accidental consequence of the fact which I have mentioned.

Pupil of one eye affected by the impression of light on the other.

Capacity of the eye not owing to the external muscles;

2. As the action of the external muscles of the eye has been frequently resorted to, for an explanation of its capacity to see objects perfectly at different distances, I requested Dr. Cutting to attend to this matter. He accordingly ascertained, while his eye was in its natural state, the distance from his face of the nearest point, at which he could make the two optic axes meet, this being the greatest trial of strength, to which those muscles can be exposed. Shortly after, he repeated the experiments, while, in consequence of the application of belladonna, he was without the power of adapting his eye to different distances, and found, that the strength of those muscles was not diminished. It follows, therefore, not only that the external muscles have little or no concern in fitting the eye to see distinctly at different distances, but that the same is true with respect to the cornea; as we cannot suppose, that its mechanical properties were altered by the belladonna, or at least, that it became more inflexible from the application to it of the juice of that herb. I had before made a similar experiment on myself, by comparing what had been the strength of the external muscles of my eyes twenty years ago *, with what it was after I had lost the power of altering their refractive state; but though I found no difference, yet, as their coats might have in the mean time become more rigid, I thought it right to have the experiment repeated, in a manner to which no objection could be taken.

but apparently to the crystallin

The only other part of the eye, or its appendages, which remains for enabling us to see equally well at very different distances, is the crystalline; and that it does produce this

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* Essay on Single Vision, &c. p. 136.

effect, either wholly, or very nearly so, is manifest, from the necessity even young persons are under, who have lost it, of using glasses of very different convexities for near and remote objects. But in what way this important office is performed by it seems still unknown. The learned Dr. Young, indeed, as well as others before him, has supposed, that the crystalline has the power of altering its figure; but the proofs hitherto given in favour of this opinion appear very defective. In 1794, I attempted to submit its justness to the test of direct experiments, by applying to the crystallines of oxen, which had been felled from thirty seconds to a minute before, chemical and mechanical stimuli, and those of galvanism and electricity; but in no instance was any alteration of figure, or other indication of muscular power, observed. All of these stimuli were applied to the crystalline while it was surrounded by air, and some of them while it was covered with warm water. Last summer, after I knew that men lose, from increase of years, the faculty of altering the refractive state of the eye, I thought it possible, that the oxen on which I had made the experiments were too old for them. I therefore repeated most of them on the crystallines of a calf and a lamb; but still no motion was to be seen. Dr. Young has made similar experiments with a similar event; but he thinks, that no argument can hence be derived against his opinion, as neither can motion be excited in the uvea, by any artificial stimulus. In the first place, however, it is not agreeable to just reasoning, to regard an unknown thing as an exception to a general rule, rather than as an example of it; in the second, the motions of the uvea are involuntary, whereas the adaptation of the eye is, in part at least, under the command of the will; and in the third, the crystalline seems very unfit for performing the motions which he assigns to it; for if its figure be altered out of the body, by external force, it does not restore itself, but retains the shape which has been given to it, like a piece of dough, or soft clay. Possibly farther experiments with belladonna may contribute to remove the obscurity which at present surrounds this subject.

though it has not been proved, that this can alter its figure.

II. *Method*

II.

Method of producing Heat, Light, and various useful Articles, from Pit-coal. By Mr. B. Cook, *of Birmingham* *.

SIR,

Products from coal.

HAVING paid much attention to the procuring of gas, and other products, from pit-coal, I now beg leave to lay before the Society for the Encouragement of Arts &c. the results of some of my experiments on pit-coal, and the methods of producing the sundry articles, of which I have sent samples, and a japanned waiter varnished therewith. The quantity of clear tar, which may be produced from every hundred weight of coal, is about four pounds; from which a liquor, or volatile oil, may be distilled, which answers the purposes of oil of turpentine in japanning. Every gallon of tar will produce nearly two quarts of this oil by distillation, and a residuum will be left nearly, if not quite, equal to the best asphaltum. I have sent a waiter, or hand-board, japanned with varnish made from this residuum, and the volatile oil above-mentioned. This dries sooner, and will be found to answer as well as the best oil of turpentine, a circumstance which will be of immense advantage to this country; as, in the vicinity of Birmingham only, nearly ten thousand tons of pit-coal are coked or charred per week; and all the tar has hitherto been lost: but by my process, I dare venture to say, that, from the various coal works in this kingdom, more tar might be produced than would supply all our dock-yards, boat builders, and other trades, with tar and pitch, beside furnishing a substitute for all the oil of turpentine and asphaltum used in the kingdom, and improving the coke so as to make iron with less charcoal.

Japan varnish.

Quantity of valuable products.

I have sent a large specimen of the asphaltum, and three vial bottles containing as follows:—

No. 1.—A sample of the oil or spirit, being part of that which was used in making the varnish, with which the waiter sent was japanned.

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* Transactions of the Society of Arts, vol. xxviii. p. 73. The silver medal was voted to Mr. Cook.

No. 2.—Is the same oil or spirit, a little more rectified.

No. 3.—The same, still farther rectified, and of course more clear, and freer from smell; but I find, that the specimen, No. 1, answers quite as well for varnish.

Price of the tar-spirit.

Tar-spirit is now about eight shillings per gallon, and turpentine-spirit about fifteen shillings, this latter has been, within the last two years, as high as forty-eight shillings per gallon, and the tar-spirit will answer equally as well for varnish, as you will observe by the enclosed certificate from Mr. Le Resche, on using the coal-tar-spirit, instead of the turpentine spirit.

It is preferable to oil of turpentine for varnish.

I requested Mr. Le Resche to use the tar-spirit just in the same way he would the foreign spirit, and then give the varnish to his work-people to use, without making any remark to them, which was done: he, making the varnish himself, found it mixed, and made the varnish as good in appearance as that prepared with the foreign spirit. He then gave the varnish to his work-people to use, and when they had finished their work with it, he found from their report, that it answered perfectly, and dried sooner; and when the waiter done with it was given to the polisher, it was found to polish much smoother under the hand, and take a more beautiful gloss than their former varnish, as the article now sent will show on inspection.

Coal-tar superior for ships.

I am of opinion, that the production of these articles will be of great public service. Permit me to add, that the timber of ships paid with this tar is not nearly so liable to be worm-eaten as those done with common tar.

I remain, Sir,
Your humble Servant,
B. COOK.

The following Certificate was received from Mr. Le Resche, who prepared and applied the Varnish of the Waiter sent to the Society.

Testimony of Mr. Le Resche.

THIS is to certify, that the spirit or oil, extracted from coal-tar, is every way adequate to the purpose for which it is intended, as a substitute for the foreign spirit or oil used in japanning.

Mr. Cook having desired me to make a trial of it, the tray, or waiter, accompanying this paper, was got up in my manufactory, and is a specimen in proof of its usefulness. The varnish used for this purpose I made myself; and, instead of mixing it with the usual spirit or oil imported, which is now become excessively dear, I mixed it with the spirit, or oil, extracted from coal-tar; and I can truly affirm, that, far from its being a substitute inferior in properties to the spirit in general use, I esteem it far superior in several respects.

In the trial I made of it, I found it would dry quicker, and the varnish mixed with it would polish with more ease, bear a good lustre, and, in short, answer every requisite purpose of the foreign spirit. If to these be added the reasonable price at which it may be sold, I cannot but pronounce it a discovery, that must eventually prove greatly advantageous to the manufacturer, as well as interesting to every lover of the arts, or admirer of talent and ingenuity.

Witness my hand, the 16th day of January, 1810,

J. S. LE RESCHE, Japanner,
Church street, Birmingham.

Reference to Mr. Cook's Apparatus for preparing Gas and and other Products from Pit-Coal, Pl. IX.

Apparatus for preparing the products from pit-coal.

A, Fig. 1, Pl. IX, is a common fire-place, a stove built with brick, having cast-iron bars to put the fire in at, and a flue that goes into a chimney; A is the cast-iron pot, (which holds from twenty-five to one hundred pounds of coal, according to the size of the premises to be lighted) which hangs by the bewels or ears on a hook, suspended by a chain in this stove or furnace, about three inches above the bars of the grate, and three inches distant from the sides of the stove; the fire then flames all round this pot, and as it does not rest on the burning fuel, it is the flame only that heats it, so that it does not scale, but will last for years. The smoke &c. are carried off into a chimney. The cover *d* of the pot is made rather conical, to fit into the top of the pot close, and from the top of the cover the elbow-pipe proceeds as far as the mark *a*. The other end of the pipe with the elbow entering the water-joint is rivetted to it after; when

Mr. Cooke's Apparatus for making Gas & other Products from Pit Coal.

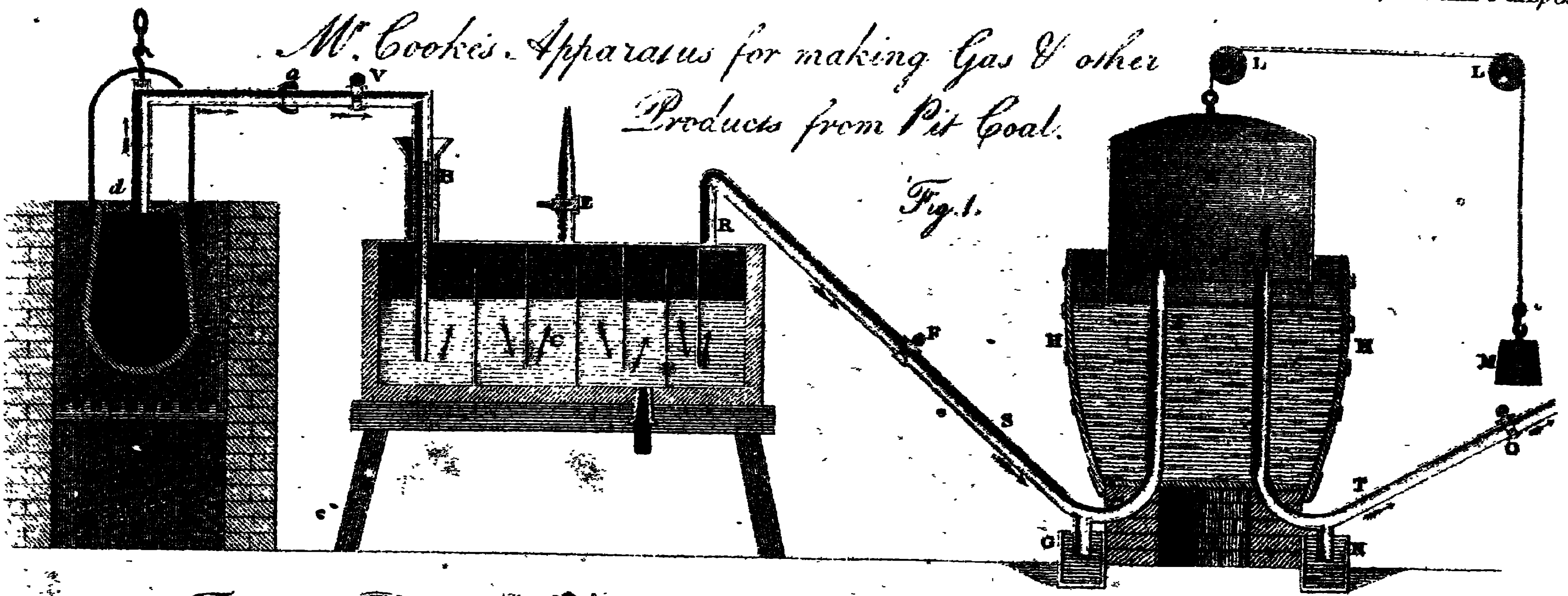
Fig. 1.

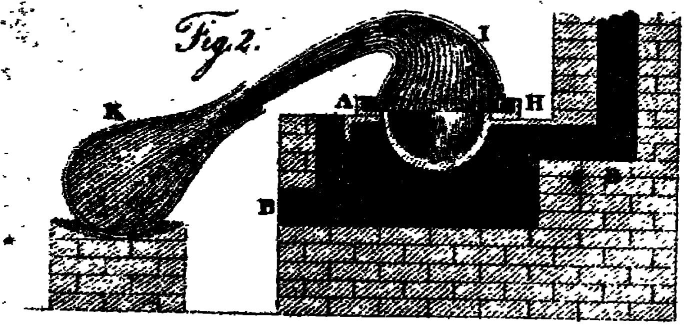
Fig. 2.

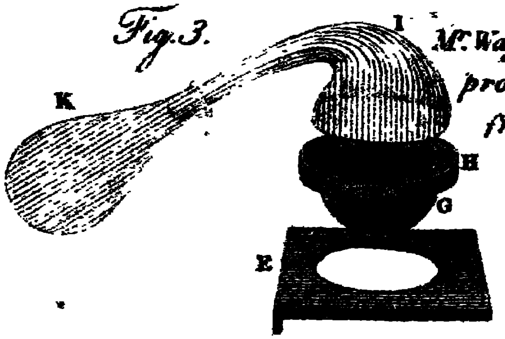
Fig. 3.

Mr. Way's method of procuring Turpentine from fir Trees.

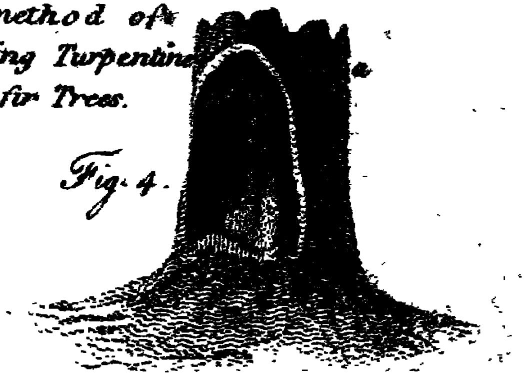
Fig. 4.

when the lid or cover of the pot is put on, the bewels or ears come over the elbow of the pipe that is on the lid, and a wedge is put between them and this elbow, to keep down the cover air-tight, and a little clay or loam may be luted in the joint, if any gas should escape round the cover of the pot. The other elbow B goes into a water-joint, formed of a tube affixed to the cover of the purifier C; and another tube, which passes through the lid of the purifier: the elbow-pipe then goes over the inner tube, and when put on, the jointing is made good by pouring water into the space between the tubes, which renders it air-tight. The gas, as the arrows show, passes down into the purifier C, which is rather more than half full of water; the use of this water-joint is for the convenience of removing the lid *d*, to which this pipe is attached. The purifier C is a wooden trough, with a sheet-iron top, to which the tubes are soldered, and it is fastened to the trough to keep all secure and air-tight. The sheets of iron, *e*, *f*, *g*, *h*, *i*, *k*, are alternately soldered to the iron top, and fastened to the wooden bottom. Now when the trough is half filled with water, the gas passes into it at B; and, as it can only find its way out again at R, it must pass through the water. The inner pipe B reaches under the surface of the water in the trough; now when the gas is forced into the water, it would rise to the top of the purifier, and go along in a body to the end, and out at the pipe R, if the sheets of iron, *e*, *f*, *g*, *h*, *i*, and *k*, which stand across the trough, with openings in them alternately at top and bottom, did not stop it, force it to descend down into the water, and hinder it from going any way but through these apertures, purifying it all the time it is passing through the whole body of water, until it is properly washed; it then escapes through the pipe R at the end of the trough C, passes down the pipe S, and is carried up into the reservoir or gazometer K. In the bottom of the purifier is an aperture, closed by a plug at D, to let off the ammoniacal water and tar as it is deposited, and the pipe, with the cock E at the top of the purifier, is to burn away the spare gas, when not to be used.

Purification of the gas.

There is a stop-cock placed in the main pipe at F, that when the reservoir is full, and gas is making, and cannot be

Method of burning the superfluous gas.

be used, the cock may be turned, and prevent any gas from passing from the reservoir, and by opening the cock E on the top of the purifier, and firing it, all the gas which is made more than is wanted for use may be burnt away. If this was not done, the gas would continue to find its way into the reservoir K, which would overflow and produce a disagreeable smell, which this simple way of burning it away as fast as it is made when not wanted, prevents.

Receptacle for the tar not left in the purifier.

It may in some measure happen, that, although the gas has passed through the purifier C, a small portion of tar will pass along with it, and would either clog the pipe S, or accumulate in the reservoir. To avoid this, there is placed at the bottom of the pipe S and G, before it rises into the reservoir, a jar, into which a pipe, made as shown in the drawing, conducts the tar; this collects all that passes through the purifier; it is filled with water, over which the gas passes up into the reservoir, but the tar drains down this lead pipe and deposits itself in the jar of water. The longer this pipe S is, the better, as it serves as a refrigitory.

Receptacle for the inflammable gas.

H is a plain cask, made to any proper size, and filled with water, with a cock to draw off the water when it becomes foul. The upper vessel K is made of sheet iron, rivetted together in the manner engine-boilers are made. If it is only from five hundred to one thousand gallons in size, it will require only two cross iron bars at top, and four ribs down the sides to keep it in form, with a strong ring at top; and as there is no stress on this vessel, it will ascend and descend easily without any other support or framing, the plain sheet iron sides being rivetted to the four ribs, and it is quite open at the bottom. A strong rope runs over the pullies L L, with a weight M to balance the vessel K, and assist it in rising and falling. The pipe J is that through which the gas passes from the reservoir or gazometer, and rising through the pipe T, is conveyed to all parts to be lighted.

Method of preventing the pipes from being clogged.

There is also another drain-pipe at N, for, after all the washing &c., a very small portion of tar and moisture may rise into the pipes, and perhaps in time clog them, but by laying all the pipes in the first, second, and third stories on a small descent, if any tar or moisture should rise, it will drain down all the pipes from top to bottom, and be deposited in the earthen jar at N, by that means the pipes will not clog up in half a century. These jars must be sometimes removed and emptied, fresh water put in, as also the water in the vessel H must be changed, to keep it clean and sweet; and the water in the purifier C should be changed every two or three days: by these means the gas will be deprived of all its smell, at least as far as washing will effect it, and the apparatus will be clean.

The water to be frequently renewed.

The stop-cock at O is for the use of a master, if he wishes to lock up the gas in the reservoir, to prevent his workman &c. wasting it in his absence; as also if any pipe should leak, or a cock be out of order, in any part of the premises, by turning this cock all the gas is kept in the reservoir while the pipe is repaired, or any other alteration made; it also extinguishes all the lights when turned, if any are left burning by careless workmen, nor can they be lighted until it is opened again.

Cock for locking up the gas, and extinguishing the lights.

The whole of this apparatus is simple, and not liable to be put out of order in such a way, but that any person may put it to rights again. All the art required to make the gas is to take off the cover of the pot, and, without removing the pot, to take out the coke, and fill it with fresh coal; wedge the cover down by putting an iron wedge between the bewels or ears and the elbow of the vessel, and, if required, plaster a little clay or loam round the cover, to keep it air-tight; a fire is then to be made under it, and the whole is done. The boy, or man, who does it, must now and then look at the fire, and keep it up, until the pot is hot, and the gas is made. Now in works where lights are wanted almost always, I would recommend two fire-places, and two pots, so that when one pot is burned out, the other pot may be ready to act; for this purpose the purifier must be provided with two of the water-joints B, one communicating with each pot, and the elbow-pipe of each pot must have a stop-cock, as V. When one pot is burning, the cock in the other pipe must be stopped, that the gas may not find its way out of the purifier; and when all the gas is extracted from that pot, the cock V, leading from it, must be stopped, and the pot left to cool; while a fire is put under the other pot, its cock is opened, and a supply of gas from it

Conduct of the process.

it is passed into the reservoir; by these means one of the pots is constantly supplying the reservoir with gas, and the lights are always kept burning. One purifier is all that is necessary. The cock V must be shut when either of the covers is taken up to fill the pot again with coal; and when the elbow-pipe is lifted out of the water-joint, as the cover is attached to it, a plug must be provided to fit into the water-joint pipe the moment the elbow is removed from it, or the gas will rush out of the pipe at the water-joint. But a better way would be, to lengthen the pipe of the water-joints B, and place a large cock under each of them, almost close to the top of the purifier; so that, when one pot was burnt out, by turning the cock it would keep all the gas in the purifier, while the cover was removed. No plug is necessary in this method. When people are very particular, (especially when houses or accompting-houses are to be lighted), and wish all smell to be destroyed, if they are not satisfied with washing it, and still think there is a little smell left, (and very little indeed, if any, will be left), after the washing, a small trough may be added, made in the same way as the purifier, with sheets of iron across to force the gas through the pipe R communicating with it. This trough may be filled with water, with a few lumps of lime put into it, and this water and lime changed often; on the gas being forced through this lime-water, if there was any remaining smell in it, this would completely take it away, and, as has been before observed, by changing all the waters now and then, and keeping this small trough constantly supplied with clean water and lime, the gas after passing it will ascend the pipes to the lights pure.

Further purification of the gas.

SIR,

HAVING been from home, I was prevented from answering your obliging letter until this day.—I am much pleased, that the Society have approved of my specimens produced from pit-coal. I also feel highly gratified and honoured with their Reward. I hope to lay before you, in a short time, an account of the establishment of a work, that will be of such magnitude, as will supply this part of the country with the oil or spirit, in sufficient quantity to supersede

Intended establishment of a large work.

sede the use of turpentine &c. in japanning; and I do hope, that in time works of the same description will be established through all Staffordshire, the products of which will supply the place of a great portion of the spirit used in the kingdom, while the pitch will be of sufficient quantity to form a great part of that article now used in the dockyards.

Requisites

All I want is support from the great coal companies and masters, to erect sufficient apparatus at the different works, to preserve the tar at all the coke furnaces, and proper means to separate the spirit from the tar. It would be a great saving to the nation, as in every hundred weight of coal coked there are lost by the present mode above four pounds of tar; and the cokes are not half so good as if they were coked in close vessels, to the exclusion of the atmospheric air. I need not describe the method by drawings of the manner of extracting the tar from pit-coal in close vessels, as that method is so generally known; it must be clear to every one, that it is procured by distilling the coal.

Present loss.

I have, as follows, described the method I use in extracting the spirit from the tar, the process of which is so simple, that every one must understand it.

Method of extracting the spirit of tar

Fig. 2, Pl. IX, is a section of the furnaces, and one of the retorts, almost any number of which may work in a line, the same flue will do for all, only taking care, if any are not at work, to stop up the draught-hole, which communicates with the flue. These furnaces are built without bars, grates, or doors. A is the place where the fewel is put in to heat the retort G; the fire lies under it, and the smoke is carried off into the flue D. B is the aperture where the ashes are raked out. G is a section of the iron basin, or lower part of the retort; the dark-shaded square part shows the space the fire occupies, and the black square D the flue as it runs along the back of all the line of furnaces, and enters the chimney R, as the arrows show. I, Figs. 2 and 3, shows the upper part of the iron, earthen, or glass retort, fitted on the cast-iron basin G. K, the receiver. By this mode of setting the retorts, all the great expense of bars, doors, frames &c. is saved, and a brisker draught of air is obtained, which may be slackened at pleasure by

Apparatus described

covering up in part, or wholly, the fire-place A with a brick. E is a square iron plate with a circular hole in the centre, built on the top of the furnace. The cast iron basin of the retort G is made to the size of the hole in the plate: the most convenient size of the basin of the retort I find is about five or six gallons, in the shape of a deep pot, with a flanch or rim H round the edge of it; this pot or basin of the retort is put into the iron plate E, and the flanch of the retort then rests on the plate E. I is the upper part of the retort without a bottom, made to rest and fit on the flanch of the cast iron basin G. K is the receiver, larger in the mouth than the nose of the retort.

Process of extracting the spirit

To begin the work, I fill, nearly, the iron basin of the retort G with coal-tar. I then put on the upper part of the retort I, and make it air-tight with a little sand thrown round it at the flanch H; the receiver K is put into its place, and a slow fire is put in at A, under the retort; the tar soon begins to boil slowly, or rather simmer. As soon as this begins, there rises from the tar a thick whitish vapour, which fills the glass retort; part becomes condensed, and falls in drops from the sides of the retort into the tar again, while the purer spirit rises into the neck, is condensed, and keeps dripping down the neck into the receiver; this is the spirit of the tar, and with this spirit that first arises from the tar was the waiter japanned which I sent you. The reason I chose to have the receiver wider at the mouth considerably than the nose of the retort is, that there is a strong and very volatile oily ammonia, that does not soon condense, but gets out of the receiver into the air the instant it leaves the retort, and though but in a very small quantity, so small that it is hardly possible to catch it; yet will it impregnate the air for a great distance round, with its very penetrating smell, while the spirit keeps dropping into the receiver pure and separate from the ammonia. The spirit is very volatile, quite as much so, if not more, than the spirit of turpentine, and soon evaporates if exposed to the air, which is a proof of its drying nature; indeed when used as a substitute for turpentine, it dries in the stove quite as soon or sooner, and takes equally as beautiful a polish. I sent you three specimens,

No. 1.

Rectification of the spirit.

No. 1. is what came off the tar first. No. 2. is the same distilled a second time: and the third specimen is the second redistilled again in a glass retort; it there leaves a little pitchy residuum, and comes over clear, as the sample. Very little of the spirit is lost in passing through these different stages, if care is taken, that the fire is slow, and the process not hurried. When the spirit is perfectly extracted from the tar, there remains in the basin of the retort that beautiful pitch or asphaltum sent; which, when mixed with the spirit, forms an ingredient for making the black varnish used in japanning. If it is wished to use it as pitch, less spirit must be extracted from it. I find, that six gallons of tar will produce, if care is taken, about two gallons or two gallons and a half of spirit. A great number of retorts may be kept working by a single man; if we say only one hundred, and only worked down in a day, they will produce from two hundred to two hundred and fifty gallons of spirit, so that by increasing the number, any quantity may be obtained. When the spirit is used in the place of turpentine, the varnish-maker uses it in the same way, and in the same quantity, as there appears no manner of difference in the use of it from the spirit of turpentine in the making of varnish. When the asphaltum is used, it supplies the place of real asphaltum, and in about the same quantity. I have explained the whole as clear as I can, but if any more information is required, I should feel happy in giving it, and am,

Asphaltum.

Pitch

Sir, With great respect,

Your obedient humble servant,

B. COOK.

Lighting apartments with gas.

*** To such persons as wish for further particulars on the subject of lighting apartments with gas, it may be proper to note, that the society, in their 26th volume of Transactions, page 202, have given an engraving and description of a gazometer, and apparatus for making carbonated hidrogen gas from pit coal, which communication was sent to them by Mr. S. Clegg, of Manchester*.

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* See Journal, vol. xxiii, p. 85. See also two original communications by Mr. Cook on the advantages of coal gas lights, even on a very small scale: vol. xxi, p. 291, and xxii, p. 145.

III.

Method of procuring Turpentine and other Products from the Scotch Fir, (Pinus Silvestris Linn.) *My Mr.* H. B. WAY, *of Bridport Harbour**.

SIR,

Extraction of turpentine from the Scotch fir in this country.

THE enormous high price of turpentine, tar, and pitch, last year, brought to my remembrance, that I had, in 1792, when in America, made some memorandums on the subject of obtaining them in North Carolina, which, on referring to, led me to think, that they might be obtained in this country. I was induced to mention it to my relation and friend, John Herbert Browne, Esq., of Weymouth, and of Sheen, in Middlesex, when on a visit at my house; and I expressed a wish, that I could try the experiment with regard to turpentine; when he very kindly gave me leave to try it on three trees growing on his estate, about three or four miles from this place, and he went with me and fixed on them, and early in last April I had them prepared for the purpose of extracting the turpentine, and they have been running till the 18th instant. The weather, except the last month and part of this, has, from so much rain falling, and there being so little hot weather, been particularly unfavourable for this business; as, the distance being such as to prevent the trees being regularly attended, the hollows were frequently found by my men full of water, and a good deal of the turpentine, which ran off with the water, lay on the ground. Under all these circumstances I was only able to obtain from the three trees about two pounds and a half of turpentine. Mr. Browne being with me again the 16th and 17th instant, as he wished to take the trees down, I begged he would allow me to take a part from one of them, for the purpose of sending to the Society of Arts, Manufactures, and Commerce, with the turpentine collected from the

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* Trans. of the Soc. of Arts, vol. xxviii, p. 86. The silver medal was voted to Mr. Way. Part of the tree, from which the turpentine was extracted, is preserved, along with some of the products, in the society's repository.

Extraction of turpentine from the Scotch fir in this country.

the trees, which he most readily complied with. I have therefore taken about six feet from one of them, (they were all nearly the same size); what I have sent is the part from the ground to the top of the place that has been cut away for the turpentine to run into the hollow, whence it was to be collected; the hollow was cut in this considerably higher than is usual in America, as this tree stood in a hedge, and could not well be hollowed lower; I have matted up this part of the tree, and secured it with straw and a double mat, to prevent the bark being rubbed off, that it may be seen in the same state as it stood when the turpentine was taken from it. The turpentine is in the cask in which it was deposited when brought from the trees; and I have this day shipped both on board the sloop Betsey, Captain Trent, bound to Downe's wharf, London, directed to you, freight paid here by me; which vessel I expect will sail in a day or two, and I hope you will receive them safe, which, when you do, you will much oblige me by requesting, that both may be examined, in the hope that this small trial may meet with the approbation of the very highly respectable and truly useful Society of Arts, Manufactures, and Commerce; and if considered likely to prove useful, that they may induce some person, who has the means and opportunity of doing it, to make a trial on a larger scale, so as fairly to ascertain whether turpentine can be obtained in this country from the very large and numerous plantations of Scotch firs, now in the United Kingdom, previous to the trees being cut down, either to thin plantations, or where ground is designed to be cleared, as taking the turpentine from the trees previous to their being cut does not at all injure the wood, and by making the hollow in the trunk of the tree about six inches from the ground, it would waste but a very small quantity of timber. I have taken the liberty of annexing a copy of memorandums I made when in North Carolina, respecting the modes of collecting turpentine, and making tar and pitch, in hopes they may afford the society some little information, as they are not, I apprehend, very generally known. They are copied from memorandums which I actually made on the spot. I would have sent the memorandum-books with this, had not the remarks been mingled with others relative to my commercial pursuits; but I shall have no hesitation in allowing any person to examine them, or to afford any information in my power to any persons willing to make experiments in this way, if they will favour me with a call. I am well satisfied in my own mind, that very large quantities of tar might be obtained from the knots and limbs of the Scotch fir when cut down; and that the charcoal made from it would not be injured by the tar being first extracted: and as I was in Norway, Sweden, and Russia, in 1789 and 1790, and saw no tree, from which I consider that tar could be extracted, except the Scotch fir, or red deal, which is one and the same tree, I am persuaded, that the refuse of that tree must be what they make the tar from in those countries, though I had no opportunity of seeing the process there. I suspect, that the Swedish tar-kilns must be constructed of brick, or some sort of masonry, as the tar brought thence is much clearer, better, and more free from extraneous matters, than that of any other country. I have observed the tar from North Carolina to have frequently a quantity of sand in it, which is easily accounted for, from the soil in which the kilns are made: it would, in the careless way in which they take it out of the hole dug in a sandy soil, be very likely to be mixed with the sand. In the small cask, in which the turpentine is, I have sent a few small red deal knots, from some timber that I have lately taken out of my warehouse, on some alterations being made; the timber from which they are taken has been in the warehouse ever since the summer of 1786, and yet, when these pieces are exposed to a moderate heat, the tar will be seen to exude from them.

Tar might be extracted from the fir made into charcoal.

Swedish tar-kilns.

North Carolina tar.

I remain, Sir,
Your obedient and very humble Servant,
H. B. WAY.

Bridport Harbour, Nov. 27, 1809.

Extracts from Notes taken by Mr. Way.

Thursday, April 12, 1792.

Method of extracting turpentine in North Carolina.

ARRIVED at Wilmington, North Carolina, about one P. M. Observed on the roads the pitch-pines prepared for extracting turpentine; which is done by cutting a hollow in the tree about six inches from the ground, and then taking the bark off from a space of about eighteen inches above it, from the sappy wood. The turpentine runs from April to October, and is caught by the hollow below. Some of the trees were cut on two sides, and only a strip of the bark left of about four inches in breadth on each of the other two sides, for conveyance of the sap necessary for the support of the tree. A Captain Cook, with whom I had been travelling, informed me, that some trees would run six or seven years, and that every year the bark was cut away higher and higher, till the tree would run no longer, and I observed many that had done running, and they were in general stripped of the bark on two sides, as high as a man could reach, and some were dead from the operation; others did not look much the worse for it. I find the usual task is for one man to attend three thousand trees, which, taken together, would produce from one hundred to one hundred and ten barrels of turpentine.

April 15, 1792.

Farther account of it.

ON my return from Wilmington to Cowen's tavern, distant about sixteen miles, I was informed, that the master of the house had been a superintendant of negroes, who collected turpentine. I found the information I had before received was not perfectly correct; he told me he attended to six slaves for a year for a planter, and between the 1st of April and the 1st of September they made six hundred barrels of turpentine. The cutting the trees for the purpose of collecting is called boxing them, and it is reckoned a good day's work to box sixty in a day; the trees will not run longer than four years, and it is necessary to take off a thin piece of the wood about once a week, and also as often as it rains, as that stops the trees running. While in North Carolina, I was particular in my inquiries respecting the making of tar and pitch, and I saw several tar-kilns; they have two sorts of wood that they make it from, both of which are the pitch-pine; the sort from which most of it is made are old trees, which have fallen down in the woods, and the sap rotted off, and is

Tar made from two sorts of wood:

lightwood, is what they call lightwood, not from the weight of it, as it is very heavy, but from its combustible nature, as it will light with a candle, and a piece of it thrown into the fire will give light enough to read and write by. All the pitch-pine will not become lightwood; the people concerned in making tar know it from the appearance of the turpentine in the grain of the wood. The other sort of wood which is used, after the trees which have been boxed for turpentine have done running, they split off the faces over which the turpentine has run; and of this wood is made what is called green tar, being made from green wood instead of dry.

and wood from trees boxed for turpentine.

Mode of making the tar.

When a sufficient quantity of wood is got together, the first step is to fix a stake in the ground, to which they fasten a string, and from the stake, as a centre, they describe a circle on the ground according to the size they wish to have the kiln. They consider that one, twenty feet in diameter, and fourteen feet high, should produce them two hundred barrels of tar. They then dig out all the earth a spit deep, shelving inwards within the circle, and sloping to the centre; the earth taken out is thrown up in a bank about one foot and a half high round the edge of the circle; they next get a pine that will split strait, of a sufficient length to reach from the centre of the circle some way beyond the bank; this pine is split through the middle, and both parts are then hollowed out, after which they are put together, and sunk in such a way, that one end, which is placed in the centre of the circle is higher than that end which comes without the bank, where a hole is dug in the ground for the tar to run into, and whence the tar is taken up and barrelled as it runs from the kiln. After the kiln is marked out, they bring the wood, ready split up, in small billets, rather smaller than are generally used for the fires in England, and it is then packed as close as possible, with the end inwards, sloping towards the middle, and the middle is filled up with small wood and the knots of trees, which last have more tar in them than any other part of the wood. The kiln is built in such a way, that at twelve or fourteen feet high it will overhang two or three feet, and it appears quite compact and solid. After the whole of the wood is piled on, they get a parcel of small logs, and then place a line of turf, then another line of logs, and so on alternately all the way up, and the top they cover with two or three thicknesses of turf. After the whole is covered in this way, they take out a turf in ten or a dozen different places round the top, at each of which they light it, and it then burns downwards till the whole of the tar is melted out; and if it burns too fast they stop some of the holes, and if not fast enough they open others, all of which the tar-burner, from practice, is able to judge of. When it begins to run slow, if it is near where charcoal is wanted, they fill up all the holes, and watch it to prevent the fire breaking out any where till the whole is charred; the charcoal is worth two pence or three pence, British sterling, per bushel. It will take six or eight days to burn a tar-kiln; in some places they burn it at such a distance from the shipping, that they have very far to roll it, and even then sell it at from three and six pence to five shillings British sterling, per barrel, sometimes taking the whole out in goods, but never less than half the amount in goods; from all which it will be reasonably supposed, that tar burning in that country is but a bad trade, as it must be a good hand to make more than at the rate of a barrel a day; the barrels cost the burner about one shilling and three pence British sterling each; the tar makers are in general very poor, except here and there one, that has an opportunity of making it near the water side.

Method of making pitch.

Pitch is made by either boiling the tar till it comes to a proper thickness, or else by burning it; the latter is done by digging a hole in the ground, and lining it with brick, it is then filled with tar, and they set fire to it, and allow it to burn till they judge it has burnt enough, which is known by dipping a stick into it, and letting it cool; when burnt enough they put a cover over it, which stops it close, and puts out the fire. Five barrels of green tar will make two of pitch; and it will take two barrels of other tar to make one of pitch.

N. B.—The foregoing observations respecting tar and pitch are copied from a memorandum made by me at Suffolk,

folk, in Virginia, on the borders of North Carolina, April 23, 1792, and are the result of the inquiries and observations I made on the subject whilst in Carolina.

Wilmington, N. C. April 13, 1792.

Pitch pine timber.

IN conversation with a Mr. Hogg, who had been settled there, and at Fayette-ville before the war, I learnt, that pitch-pine timber growing on the sands was the best; and that it was reckoned to be better if cut in the winter, before the sap rises in the tree.

H. B. WAY.

SIR,

Experiments on a large scale recommended.

Venice turpentine.

The timber improved by it.

IT affords me much pleasure to learn, that my communication, on the extraction of turpentine from the Scotch fir, has been thought worthy of the consideration of the society; and it will be highly gratifying to me, if it should induce persons, who have considerable plantations, to try it on such a scale, as to ascertain to what extent it might prove beneficial in this country. The experiment should be tried on trees so situate as to be conveniently examined every day, and the turpentine collected into the hollows removed as often as possible to prevent its being injured, or wasted by the rain. I think, that during the American war, some importations of turpentine were made from Russia and Sweden; and if so, it must have been extracted from what we call the Scotch fir in a colder climate than this. The article called Venice turpentine, which is brought from Carinthia and Carniola, is extracted there from the larch tree; and it might probably answer to try to produce it from the larch trees grown in Great Britain, in the same way as I have collected the turpentine from the Scotch fir. Respecting the wood of the Scotch fir being injured, by the extraction of the turpentine from it, I should rather think, that it would, on the contrary, be better for it; as all those who use deals from Scotch fir in this neighbourhood complain, that it is too full of turpentine to work well. The fact might be ascertained, by the piece of timber which I sent to the society; as, if it was wished to preserve that part in which the hollow is made, the back part, or nearly half of the tree, might be sawn into boards without injury,

jury, and these boards might be compared with some from a tree taken down in the winter, from which the turpentine has not been extracted. It must, however, be noted, that from the tree I have sent to the society, the turpentine has only been running one year, whereas, in America, they collect the turpentine from the same tree for three or four succeeding years. It has been supposed and asserted, that turpentine was only obtainable from the United States; but I have sufficient documents to prove, if required, that a very large quantity of it can be procured from East Florida; and I well remember, that about the year 1782, several cargoes of turpentine were shipped in the river St. John's, for Britain; and though that country is at present in the hands of the Spaniards, no doubt, arrangements might be made with the Spanish government for a supply of that necessary article thence. It is my earnest wish, that, through the medium of the Society of Arts, I may render any information that may be serviceable to the interest of the united empire; and I will, with pleasure, furnish farther communication on the products of Florida and its commerce, if desired by the society.

Turpentine may be procured in large quantity from East Florida.

I am convinced, that tar might be produced from the refuse of firs of English growth to advantage; and that a much better article might be made from them in Britain, than any imported from America. The Scotch firs, in England, from being planted at a greater distance from each other than they are naturally found abroad, have much larger knots, and greater numbers of them, than in Carolina, or the north of Europe, and would therefore produce more tar, in proportion, from their refuse of wood, than the trees of those countries.

Tar might be obtained from the refuse of British firs.

The pitch-pine of Virginia, the Carolinas, Georgia, and the Floridas, grows to an immense size in what are there called pine barrens, the soil of which is finer and whiter than the sand used as writing-sand in Great Britain, and the trees grow almost to the verge of high-water mark on the sea-shores. I think it would answer a good purpose for the society to encourage, by premiums, the extraction of turpentine from British firs. I remain,

Pine-barrens.

Sir, Your obedient, and very humble Servant,

H. B. WAY.

J. H.

Testimony of Mr. Browne.

J. H. Browne, Esq., of Weymouth, certified, that he had witnessed the principal experiments made by Mr. Way, in extracting the turpentine from the Scotch firs. That the trees had been planted in 1771 or 1772; and that the wood, subsequent to the operation, had been minutely examined, and found not to be injured by the extraction of the turpentine. He added, that the season was uncommonly wet and unfavourable for the experiment.

Reference to the Description of Mr. H. B. Way's Method of procuring Turpentine from Fir Trees. Plate IX, Fig. 4.

a, Represents the lower part of a fir tree, as growing in the earth; *b*, shows the part where a portion of the bark is taken off to assist the emission of the turpentine; *c* is a hollow cut within the body of the tree, it is in the form of a basin at the lower part to receive the turpentine, which exsudes into it from the pores of the tree; this basin is about six inches from the ground.

IV.

Analysis of Deadly Nightshade, Atropa Belladonna; by Mr. VAUQUELIN*.

Deadly nightshade examined for the acid principle of tobacco.

THE experiments I am about to relate were instituted for the purpose of knowing, whether this plant, which is of the same family as tobacco, contained the acrid principle, that we found in the latter †; but we shall see below, that it does not exist in them. However, I availed myself of this opportunity to examine the properties of the matter in this plant, which, according to the physicians, is narcotic.

The juice

1. The expressed and filtered juice of belladonna has a pretty deep brown colour, and a bitter nauseous taste.

coagulated by heat.

It is copiously coagulated by heat, and by an aqueous infusion of gall.

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* Ann. de Chim. vol. lxxii, p. 53.

† See Journ. p. 260.

2. The

The coagulum.

2. The substance coagulated by heat in the juice of belladonna is of a yellowish gray, becomes black by dessication, and presents a smooth polished fracture like that of resins. It burns with decrepitation, softening, and smoke, which has the same smell as horn similarly treated.

The juice distilled.

3. The juice of belladonna, distilled till it is reduced to the consistence of a liquid extract, yielded a water, which had only a flat herbaceous taste, and nothing of the acrimony of that of tobacco. The only reagent, of all we tried, that rendered it slightly turbid, was acetate of lead.

The residuum treated with alcohol.

4. The juice reduced to the consistence of an extract being treated with alcohol, part dissolved in it; and the solution deposited on cooling crystals of nitrate of potash, and a little muriate of the same base.

Spirituous extract.

The alcohol, separated from these crystals and evaporated, left as a residuum a brownish yellow matter, of a very bitter and nauseous taste; which, taken up a second time by highly dephlegmated alcohol, left a fresh quantity of insoluble matter, and still deposited a few crystals of the same salt.

The matter being divested as much as possible by this process of the greater part of the nitre, and of the substance insoluble in alcohol, I evaporated the alcohol, and subjected its residuum to the following experiments:

Its properties.

1. It dissolves abundantly and speedily in water, and is even deliquescent in the air.

2. The solution is of a yellowish brown, and has a very bitter and very disagreeable taste.

3. It reddens litmus paper very deeply.

4. It is copiously precipitated by alcoholic tincture of galls, and not by acetate of lead, if the latter be sufficiently diluted with water; but, as it contains a little muriate of potash, it would precipitate the acetate of lead without this precaution.

5. This solution, when mixed with sulphuric acid, emitted a very evident smell of acetic acid.

6. The same solution, on the addition of nitrate of silver, threw down a true muriate of silver.

7. Caustic potash produces from the solution a fetid smell very similar to that of stale lie, in which linen has been

washed, and which is beginning to putrefy. Ammoniacal vapours too arise, which may be rendered sensible by weak nitric acid held at a little distance from the mixture.

8. The addition of a few drops of sulphate of iron renders the solution of a much deeper colour.

9. The extract itself, exposed on burning coals, swells up, and emits pungent and acrid fumes, in which the smell cannot distinguish ammonia.

Its contents.

From the effects produced on the solution of extract of belladonna by the various tests employed above we may conclude, that it contains, 1, a free acid; 2, an alkaline muriate; 3, a small quantity of an ammoniacal salt.

The acid the acetic.

The acid that exists in it must be the acetic, since sulphuric acid elicits the smell of this acid, and acetate of lead occasions no precipitate; which it would, if the acid were the malic, tartarous, or oxalic. Part of this acid must be combined with potash; and it is this, no doubt, that communicates to the extractive mass the property of attracting the moisture of the air.

Its poisonous qualities not owing to the salts or acid.

But neither these salts, nor these acids [this acid], impart to the matter its poisonous qualities. These unquestionably reside in the vegetable substance itself. What then is the order of composition, that makes thus, with the same principles, both our food and such deadly poisons? This is one of the barriers, that chemistry has not yet been able to overstep; and unfortunately beyond this barrier lie secrets of the utmost importance to mankind. Wanting therefore the means, which at some future period may give us a precise knowledge of the differences, that exist between vegetable compounds possessing such opposite properties, we must have recourse to observation of their effects.

Destructive distillation of the extract.

One of the methods, that appeared to us best adapted to elucidate the nature of that substance in belladonna which is soluble in alcohol, was its decomposition by fire. Accordingly I introduced 2·7 gr. [41·7 grs.] into a glass retort, and heated it gradually, till the water of solution had been distilled over by a very strong heat. A yellow ammoniacal liquid passed over, and afterward a thick oil, which had a very singular disagreeable smell.

On examining the liquid product I found a great deal of free ammonia, though there was some in combination, for the addition of a few drops of caustic potash rendered the ammoniacal smell much stronger. The oil was black, very thick, and very acrid.

The coal.

The coal left in the retort weighed 1 gr. [15·45 grs.]. It had an alkaline and prussiate taste. Washed with boiling water it yielded a lixivium, which, being mixed with sulphate of iron, produced a very considerable quantity of prussian blue, considering the small quantity of matter employed. The coal, after having been lixiviated and dried, still weighed 7 dec. [10·81 grs.].

Its quantity very great.

This quantity of coal, exclusive of what was encrusted on the retort by the violence of the fire, which I was unable to separate, is larger than is furnished by most other vegetable matters, that I have yet had an opportunity of distilling: for the 2·7 gr. [41·7 grs.] certainly contained more than 7 dec. [10·81 grs.] of water, beside nitrate and acetate of potash.

It contains much hidrogen and nitrogen.

It appears too, that it contains a large quantity of nitrogen and hidrogen, since it yielded by distillation a great deal of ammonia, prussic acid, and oil. But as this matter might contain a little nitrate, I suspected, that part at least of the nitrogen forming the ammonia and prussic acid was produced from the nitric acid.

Gum arabic mixed with nitre and distilled.

To clear up this doubt, I mixed 6 gr. [92·67 grs.] of gum arabic, in which there is supposed to be nitrogen, with a tenth of saltpetre; and, after having distilled, examined the products. The liquid that came over was in fact ammoniacal; and its smell became stronger on the addition of potash, which shows, that an acid was formed at the same time with the alkali.

The coal remaining in the retort, which weighed 2 gr. [30·89 grs.], and was extremely pyrophoric, contained prussiate of potash, like that of my matter. But though I employed in this experiment three times as much gum, and probably more saltpetre, the mixture did not furnish so large a quantity of ammonia, or of prussic acid, as the nauseous principle of belladonna.

Much prussic acid and ammonia furnished by the extract.

If we admit therefore, that the saltpetre contained in the 2 gr. of this principle gave rise to some prussic acid and ammonia, we ought not to infer, that the vegetable matter in question furnished none. That it did is the more probable, because its solution was precipitated by infusion of galls. Be it as it may, this experiment shows, that it is difficult to judge from distillation, whether organic matter containing saltpetre be of a vegetable or animal nature.

It abounds in combustible radicals,

The results of this analysis, though hitherto very rude, are sufficient however to show, that the substance, which constitutes the subject of them, contains a great deal of charcoal, hidrogen, and nitrogen, and but little oxigen, if we may judge from the small quantity of carbonic acid formed during its decomposition by fire.

which are probably the cause of its effects,

From what has been said may we be allowed to suppose, that the narcotic effects, which belladonna produces in the animal economy, are owing to the superabundance of combustible radicals, and particularly to that of the charcoal over that of the oxigen in the principle of this plant soluble in spirit of wine? Without pretending to assert this, it is nevertheless certain, that all the vegetable matters, which produce analogous effects, are rich in charcoal, hidrogen, and nitrogen, while substances greatly oxigenated produce contrary effects.

particularly assisted by nitrogen.

It must be confessed too, that a great many vegetable products equally abundant in these two principles do not possess the same qualities; but the nitrogen, which is always found associated with hidrogen and charcoal in the somniferous plants, does not exist, at least in similar quantity, in the others.

Examination of the part of belladonna insoluble in alcohol.

Part insoluble in alcohol examined.

1. This matter dissolved in water communicates to it the property of frothing when shaken.

2. The solution is copiously precipitated by aqueous infusion of galls.

3. Nitrate of barytes causes in it a precipitate partly soluble in nitric acid.

4. Muriate of lime produces a precipitate wholly soluble in nitric acid.

5. The solution reddens litmus paper.

6. Nitrate of silver produces in it no effect.

7. Burned in a crucible it leaves an alkaline and hepatic coal.

Its component parts.

From these effects we may conclude, that this part of belladonna is composed of an animal matter, of sulphate of potash, of acidulous oxalate of potash, probably of nitrate, and that it contains no muriate.

We may conclude too from these effects, that no earthy salts are present in it, since muriate of lime, as well as nitrate of barytes, produces in it a precipitate.

I satisfied myself by trials made on a larger scale, that the precipitates occasioned in the solution of the substance in question by nitrate of barytes were, the first, oxalate of lime, the second sulphate of barytes.

Oxalate of lime has a strong attraction for animal matter.

The oxalate of lime had carried down with it a large quantity of animal matter, which gave it a brown colour. This indicates, that this salt has a powerful attraction for animal matter; and explains why mulberry calculi, which are known to be composed of oxalate of lime, have a much deeper colour than other calculi.

No gum in it.

After having precipitated successively, as I have said, the sulphate of potash, and acidulous oxalate of potash, I evaporated the liquor, which was still coloured, and contained nitrate of potash and muriate of lime; and I treated it with nitric acid, to know whether it contained any gum: but, as I could not obtain an atom of sacchlactic acid, I concluded, that it contained none. It was formed only of oxalic acid and a yellow matter. This substance appeared then to be entirely of an animal nature.

Matters contained in the juice of belladonna.

From what has been said we find, that the juice of belladonna contains the following matters:

1, An animal substance, which is partly coagulated by heat, and partly remains dissolved in the juice by means of the free acetic acid present in it.

2, A substance soluble in spirit of wine, which has a bitter and nauseous taste, by combining with tannin becomes insoluble, and furnishes ammonia when decomposed by fire.

3, Several salts with base of potash, namely, a great deal of nitrate, some muriate, some sulphate, acidulous oxalate, and acetate.

The woody part.

The substance of the belladonna, from which the juice had been expressed, having been washed with hot water, dried, and then burned, left ashes composed of a pretty large quantity of lime, phosphate of lime, iron, and silex.

This lime announces, that the plant contained oxalate of lime, which had been decomposed by the fire.

Its properties owing to the part soluble in alcohol.

There can be no doubt, that it is the matter in belladonna soluble in alcohol, which alone produces its deleterious effect on the animal economy; for it is the only sapid part, and the well known effects of all the other matters accompanying it in no respect resemble those of the plant.

This proved on a dog.

To place this beyond doubt, I gave a middle-sized dog a certain quantity of this principle mixed with his food.

Experiment 1.

A quarter after twelve I made this dog take 1 gr. [15.45 grs.] of the extract rolled up in 10 gr. [154·5 grs.] of bread and meat made into a paste.

Effects.

In three quarters of an hour the animal appeared inclined to sleep; he held his head down, and seemed unable to support it; he lay down several times with his head on the ground; his paws were slightly convulsed; his jaws moved for some time, as if he were chewing. These effects continued about three quarters of an hour, but nothing farther ensued, and the dog resumed his ordinary manners.

Experiment 2.

At 2 o'clock in the afternoon I gave him 2 gr. [30·89 grs.] of the extract in 12 gr. of paste. The effects were renewed; but they were slighter, and of shorter duration.

Experiment 3.

At 3 o'clock I made him swallow 4 gr. [61·78 grs.] of the same extract, with about 30 gr. [363 grs.] of paste.

Effects.

A few minutes after he was seized with a continual but uncertain and difficult movement; chiefly in the abdominal extremities; and uttered some plaintive cries.

At half after three he found great difficulty in moving himself; he dropped frequently on his hind feet; and his respiration was very much confined. He attempted several times to go through the wall, which showed a sort of delirium. He had then a trembling in all his muscles.

At a quarter after four he lay down, and appeared in a profound sleep. His pulse was too quick to be counted.

At half after four he vomited up the paste he had taken; and some time after he rose, but walked with difficulty, falling sometimes on one side, sometimes on his hind legs.

He carried his head very low, his eyelids drooped, and he no longer distinguished objects; at least he struck himself against the walls and furniture of the laboratory in walking. His nostrils were scarcely sensible to the vapour of ammonia; and his ears heard nothing, for the most sudden noise did not make him stir.

He had not lost his memory however; for having put him, with a view to give him some vinegar and water, into the same posture as when he took the paste, he flew into a dreadful rage, as if all his strength had been at once renewed. From that time the symptoms he had experienced imperceptibly diminished, and about 8 o'clock at night he had recovered all his outward senses, but was still greatly fatigued. The next day he ate as usual.

Narcotic and intoxicating

Such are the phenomena this animal exhibited: and every one must perceive in them the effects of narcotism and intoxication carried to their extreme; whence resulted a sort of delirium. It is probable, that if he had not brought up the greater part of the matter, before it had time to produce its effect, it would have killed him.

V.

On the Use of Sulphate of Soda in the Fabrication of Glass: by Mr. MARCEL DE SERRES, *Inspector of Arts, Sciences and Manufactures* *.

Sulphate of soda employed in glass-making.

MY object is to give some account of the attempt made by Dr. Gehlen to employ the sulphate of soda in glass-works; and as I have had an opportunity of seeing the results of his experiments, and conversing with him on the subject of those, which he still intends to make, on the different substances that may be employed in glass-houses, I conceive, that the following particulars will not be uninteresting. They who wish for information more at large may find it in the work which Dr. Gehlen has lately published, entitled *Beytrage zur wissenschaftlichen Begruendung der Glasmacherkunst*, Attempt to establish the Art of Glassmaking on Scientific Principles, Munich, 1810.

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* Abridged from Ann. de Chim. vol. lxxvi, p. 172.

From a number of experiments, made in the large way by Mr. Francis Baader and Dr. Gehlen, it appears;

General observations.

1, That sulphate of soda perfectly freed from its water of crystallization, may be very successfully employed in manufacturing fine white glass, without the addition of potash or soda.

Advantages.

2, That in using this flux there is a considerable gain in point of time; and consequently in the product of a given furnace, and in materials. These advantages arise from a larger quantity of silex being dissolved by sulphate of soda freed from its water of crystallization.

Requires considerable care.

3, That it only requires great accuracy in the addition of the quantity of charcoal necessary to effect the decomposition of the sulphate of soda. This is so essential, that sometimes a single hundredth part too much, or too little, almost spoils the vitrification, or colours the glass. It must be observed too, that it is difficult to give precise directions for the quantity of charcoal to be employed, because the proportion must vary according to its dryness or moisture. If it be moist, it will yield more carbonic acid, which cannot certainly be advantageous to the vitrification.

Should be first decomposed into a sulphuret.

4. That sulphate of soda cannot be employed so well in substance in the melting pots; but that it is better first to make a sulphuret of soda, in order to get rid of the large quantity of carbonic acid, which is formed in the disoxidation of the sulphuric acid, and would cause too great an effervescence in the melted matter.

Glass-gall.

5, That the glass-gall is decomposed by an addition of charcoal in all the other manufactures of glass, which is a great advantage, because this gall is the greatest enemy to the manufacture of fine glass.

Peculiar attention to the pots.

6, That the pots, in which the glass is melted by means of sulphate of soda, must be made with much care, and with a different proportion of materials, because this glass attacks them much more than that made with potash.

7, That sulphate of soda may be very well prepared by decomposing muriate of soda; and for this purpose the waste of vitriol manufactories may be employed, which is a considerable saving. Preparation of the sulphate.

8, Lastly, it is well known, that, when fine glass is made, and more soda or potash is mixed in it than in common glass, the glass, if not properly cooled before it is wrought, though at first very pure, begins soon to enter into fermentation while working, and afterward appears full of blebs. It is observable, that glass made with feldspar containing potash always abounds in blebs; yet it is possible, to make good glass of it, and thus turn to account the potash contained in it. Blebs in glass.

Experiments.

As the sentiments of Kreschmann, Pott, Laxmann, Gren, Lampadius, Van Mons, and Pajot-Descharmes, respecting the use of sulphate and muriate of soda in the fabrication of glass, differ widely, it was necessary to make the following experiments, to ascertain the processes, that might answer. Experiments.

1. First a mixture of quartz and sulphate of soda, in the proportions of 100 to 60, was made, and exposed to the fire of a glass-house furnace twenty-two hours. At the end of this time no virtification had taken place, or at least it was imperfect, however high the heat was carried. Experiment 1.

2. Quartz, sulphate of soda, and burnt lime, were taken in the proportions of 100, 100, and 15, and heated. A second mixture was made in the proportions of 100, 50, and 20; and a third in the proportions of 100, 54, and 17. The third mixture was heated in a furnace the fire of which was urged by bellows. At the expiration of four hours more vitrification had taken place, it is true, than in the first experiment; but the glass was very stiff, and as it were stony. Experiment 2.

3. Quartz, calcined potash, lime, and sulphate of soda, were mixed in the proportions of 100, 10, 17, and 43, and Experiment 3.

at the expiration of an hour and a half the result was the same.

Experiment 4. 4. Quartz, sulphate of soda, lime, and charcoal dust were mixed in the proportion of 100, 54, and 14, for the former three; and the charcoal was varied from 4 to 4·2, 4·4, and 4·5. These mixtures were left in the fire an hour, and a brownish yellow or sometimes colourless glass was obtained, the colour always depending on the proportion of charcoal employed.

Experiment 5. 5. In the 5th experiment quartz was mixed with sulphuret of soda, obtained from carbonate of soda and sulphur heated together till no more sulphur was sublimed, in the proportion of 100 to 60:

Experiment 6. 6. In the 6th quartz was mixed with sulphuret of soda, obtained from eight parts of calcined sulphate of soda and one of charcoal dust, and lime, in the proportions of 100, 45, and 17:

Experiment 7. 7. In the 7th quartz was mixed with sulphate of soda, sulphuret of soda, and lime, in the proportions of 100, 24, 24, and 17; and also in those of 100, 2·5 or 3, 45, and 17. The mixture was left in the fire an hour. These experiments gave the same result as the 4th. When these trials, and many more, the particulars of which it is unnecessary to recite, had been made, the process was attempted in the large way. The mixture was formed of 100 parts quartz, 54 sulphate of soda, 17 lime, and 5 charcoal. During the fusion, a shovelful of burning charcoal from the furnace was thrown in, the five parts proving too little in the circumstances, that took place in the glass furnace. The general results of these experiments were:

General results of the experiments. 1. That sulphate of soda may be employed in glassmaking, without any addition of potash or of soda. The glass obtained by this process is as beautiful and as white as glass made with the usual materials, and has all the same qualities.

2. That the vitrification of sulphate of soda with quartz is very imperfect even in the strongest fire. It is more complete, if lime be added, but then it requires a great deal of time and fuel: and it is rendered perfect by the help of a substance, that decomposes the sulphuric acid of

the sulphate of soda, and thus removes the obstacle, that prevents the soda from acting on the silex. The best medium that can be employed is charcoal, or for flint glass metallic lead.

Precautions necessary.

This decomposition may be conducted during the vitrification, or previous to it. The methods employed must be varied according to circumstances, but it is essential to observe, 1st, the property charcoal has of colouring glass, even when in very small quantity; this property of charcoal not being exceeded by any of the metallic oxides hitherto known: 2dly, the preference to be given to lime reduced to powder, dissolved in water, and heated anew, before lime slacked in the air: 3dly, the great effervescence of the glass, when sulphate of soda is employed, an effervescence, however, not greater, than sometimes arises from common soda; and hence the precaution that must be taken to add it in smaller successive portions, than if potash were employed: 4thly, that the work must be carefully distributed in glasshouses of this kind, not to be troubled by this effervescence: 5thly, that sulphuret of soda may be more useful in glassmaking than sulphate of soda: and lastly, care must be taken in preparing the pots, because the sulphate of soda has a particular effect, as every other flux has.

VI.

On the Cause of the Refrigeration observed in Animals exposed to a high Degree of Heat: by FRANCIS DELAROCHE, *M. D.* *.

The animal economy subject to peculiar as well as physical laws.

THE animal economy presents us with phenomena, which, differing in their nature from those exhibited by inorganic bodies, cannot be explained by the ordinary results of the laws of physics; while at the same time it produces others, which, being more or less similar to physical effects, are apparently derived from the same laws. Some physiologists, it

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* Journal de Physique, vol. lxxi, p. 289. Read to the Institute the 6th of November, 1809.

Mechanical philosophy has been carried too far.

it is true, struck with the errours committed by those, who have had a rage for ascribing every thing to mechanical laws, will not admit any explanation of this kind in the animal economy. They are of opinion, that the phenomena, essentially connected with the exercise of life, must depend on the laws that govern vitality; and not on physical laws, which have little apparent connection with the former, and very frequently seem in opposition to them. But is not this opinion founded on reasoning rather than experiment? And if some of the phenomena of life appear to be contradictory to those laws, to which inanimate bodies are subject, must we thence infer, that it is the same with all of them? This reasoning, erroneous in itself, would be contradictory to experience. Who, indeed, can overlook the influence of physical causes in several of the phenomena of the animal economy; such for instance as distinct vision, which depends essentially on the refracting powers of the humours of the eye; or the movements of our limbs, in which our bones act as levers, our tendons as cords? It is true, that physical causes alone are not sufficient to produce these results, and vital causes* powerfully concur in them; but the influence of the former is not the less evident. Generally speaking it may be said, that there is scarcely a phenomenon of the animal economy, which is not owing to both. Sometimes the influence of physical causes is predominant, at others that of the vital; and frequently it is difficult to determine with precision what belongs to one, and what to the other. It is of no small consequence, however, to attain this object; and the researches capable of leading to it may be ranked among the most important in physiology. If we can ever hope to acquire

But not wholly to be rejected.

Commonly both act:

sometimes one predominating, sometimes the other.

Vital causes.

* When I speak of vital causes and vital laws, I do not mean to assert, that they are actually different from the general laws, that govern inanimate matter, and independent of them: they are, perhaps, only modifications of them; but I am of opinion, that, in the present state of science, we must admit them, if we would acquire tolerably accurate ideas of the mode, in which the different functions of organic bodies are executed. We are yet far from having reached the period, when many of the phenomena exhibited by these bodies may be referred to the laws of physics.

quire precise notions of the vital powers, and how they differ from physical, it must be by observing what is peculiar to them in the vital functions, not by vaguely ascribing to them all the phenomena of organic bodies.

Animals exposed to a high temperature generate cold,

One of the phenomena, in which it seems to me most easy to make the distinction, is that exhibited by animals exposed to a high degree of heat. It is well known that they then assume a temperature much below that of the surrounding medium. It is near half a century since this remarkable faculty in animals was noticed; and it has subsequently given rise to various experiments, particularly those made conjointly by Sir J. Banks, Sir C. Blagden, Dr. Fordyce, and some other philosophers: but we have not yet any precise ideas of its cause, which some suppose to be the refrigeration produced by the evaporation of the perspirable matter, others the same with that of animal heat, whether they imagine themselves acquainted with this, or believe it to be yet unknown. Some considerations on this question will form the subject of the present paper: but I think it necessary in the first place to repeat an observation, which I made some years ago*; this is, that we form a greatly exaggerated notion of this phenomenon, when we suppose the faculty of producing cold in animals is as striking, as that of producing heat. I believe I have proved, that this opinion, which has generally prevailed since the publication of the experiments abovementioned, is altogether erroneous. In fact, in a number of experiments made in common with my friend Dr. Berger, I constantly found, that the temperature of animals exposed to a higher heat than 35° or 40° cent. [95° or 104° F.] rose in a very striking degree, without however reaching that of the surrounding medium. I frequently observed, that this rise of temperature amounted to 6° or 7° [10·8° or 12·6° F.]; and I even ascertained, that, when the external heat is very considerable, this increase of temperature has

but not in such a degree as is commonly supposed.

Their heat increased

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* In my inaugural dissertation, entitled Experiments on the Effects that a high Degree of Heat produces in the Animal Economy. See, Collection of Theses of the Medical School at Paris, for 1806, No. 11.

without limit.

no limit but the death of the animal, which is its necessary consequence. In these experiments I ascertained the temperature of the animals by a method, of the accuracy of which there can be no doubt; that is, by introducing a considerable way into the rectum the bulb of a thermometer purposely made very small. I found a similar increase of temperature in the human subject, by means of a thermometer introduced into the mouth: and I even observed it very strikingly in a case, where the head could be affected only by means of the circulation; that of a person placed in a box filled with hot vapour, but having his head out of it.

A man in hot vapour with his head free

The faculty of producing cold real.

It follows from these facts, that the faculty of producing cold is much more limited than is commonly supposed; not that it is imaginary. Too many facts attest its existence, for it to be doubted: it is desirable, therefore, to ascertain its cause; and this I shall attempt to do.

Its cause supposed to be the same with that of producing heat.

I have said above, that some suppose this cause to be the same with that of animal heat; and they ground this opinion on the results of the experiments of Blagden and Fordyce, from which it would seem we may infer, that animals preserve a uniform temperature, whatever may be the heat of the surrounding medium; and that consequently their faculty of producing cold is as decided as that of producing heat. Indeed, if it were thus, it would be natural to conceive this uniformity of temperature as one and the same phenomenon, originating from a single cause: but this not being the precise fact, as I have shown, we may presume the conclusion to be erroneous.

This is a mistake.

Proof from a similar faculty in cold blooded animals.

There is one observation, that tends strongly to support this opinion. It is, that cold-blooded animals possess the faculty of preserving a temperature below that of the surrounding medium, when this is elevated, as much or more than warm-blooded animals: though, if this faculty arose from the same cause, as that which produces animal heat, cold-blooded animals should be nearly destitute of it. The truth of this assertion I have shown by several experiments in the paper already quoted; and the following, which I lately made, appear to me to render it unquestionable.

In a stove I exposed to a mean temperature of 45° [113° F.] a rabbit, the temperature of which before the experiment was 39·7° [103·46° F.]. After remaining there an hour and forty minutes, it had acquired a temperature of 43·8 [110·84° F.]. A frog, exposed in the same stove to a similar heat, acquired in one hour a temperature of 26·7° [80·06° F.]; which it preserved during the rest of the time it remained in the stove, being half an hour. The temperature of another frog, exposed to a mean heat of 46·2° [115·16° F.], rose to 28° [82·4° F.], at which it became stationary.

Experiments on a rabbit

and two frogs.

They who have imagined, that there was no necessary connection between the cause of animal heat, and that of the cold sometimes produced in the animals, have supposed, that the latter might be occasioned by the evaporation that takes place, either at the surface of the body, or in the lungs; thus comparing this phenomena with the cooling of inanimate bodies, the surface of which is wet. For this ingenious comparison we are indebted to Franklin; but is it just? The only experiments made till lately, with a view to solve this question, those of Sir C. Blagden and his coadjutors, and those of Dr. Crawford, seem to indicate, that it is not. Those I made myself a few years ago, and of which I have given an account in the thesis already quoted, led me, on the contrary, to adopt the supposition of Dr. Franklin; though they did not allow me to form a decisive opinion. I have since attempted some new ones, which, confirming the results I had before obtained, appear to me calculated to remove all doubts on the subject. Of these I shall give the results preceded by a brief account of those I formerly published.

Some have supposed the cold produced by evaporation.

This contradicted by some experiments;

confirmed by others.

The principal object of the latter was to ascertain the validity of the objection commonly made to Franklin's theory, that the cooling produced by evaporation is insufficient, to explain the difference observed between the temperature of animals exposed to a high degree of heat and that of the surrounding medium. To determine this it was sufficient, to examine the comparative influence of heat on the temperature of animals, and on that of inanimate substances wetted all over. For this purpose I exposed at the

The insufficiency of this cause has been alleged.

Comparative experiments.

same time, and side by side, in a stove, various animals, alcarrazas filled with water, and wet sponges. In making this experiment, which I have several times repeated, I constantly observed, that the alcarrazas and sponges, whether I introduced them into the stove cold, or previously warmed,* assumed a temperature below that acquired by the warm-blooded animals, but nearly the same as that of the cold-blooded *. From these results then we may infer, that evaporation is sufficient, to produce a refrigeration as great, if not greater than that observed in animals; and hence we may presume, that it is the cause of the latter. It would be wrong however, to consider the latter as a necessary consequence of the preceding proposition. The possibility of a thing is not a sufficient ground for our concluding, that it actually is. Accordingly, when I publish-

Evaporation an adequate cause.

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* To render the experiment completely accurate, it would have been necessary to ascertain the final temperature, that would have been acquired both by the animals and the inanimate substances, when the heat had produced its utmost effect on them. This I found very difficult with respect to warm-blooded animals: a long continued heat exhausting them greatly, I satisfied myself with an approximation to the limit. I generally waited, till the inanimate substances had attained it; which was much more easy, because I took care previous to the experiment, to raise their temperature nearly to the point, at which it would ultimately arrive by their exposure in the stove.

I shall here give the result of two experiments of the same kind, lately tried.

Results of experiments.

I enclosed in one basket, separating them only by an open partition, a rabbit, and an alcarraza full of water; and placed them in a stove, the mean heat of which, during the experiment, was 45° [113° F.]. The temperature of the rabbit, when introduced, was 39·7° [103·46° F.]; that of the alcarraza about 35° [95° F.]. The temperature of the rabbit gradually rose to 43·8° [110·84° F.]; that of the alcarraza, on the contrary, fell to 31·4° [88·52° F.], at which point it appeared to continue stationary.

In the second experiment I exposed, in the same stove, to a mean temperature of 36·5° [97·7° F.], two small sponges and a frog. The latter, which was placed between the two sponges, acquired, in the course of an hour, the stationary temperature of 28·2° [82·76° F.], the sponge on the left that of 27·9° [82·22° F.], and the sponge on the right that of 27·6° [81·68° F.].

ed these results, I did not pretend to decide, that evaporation was the true cause of the phenomenon in question; I merely held out this opinion as plausible: at present I think I can give direct proofs of its justice.

Test of its efficacy.

If evaporation be the sole cause that produces the refrigeration of animals exposed to a high degree of heat, it is evident, that by suppressing it both on the surface of the body and in the interior of the lungs, this refrigeration will be prevented, and the animals must acquire a temperature equal or superior to that of the medium, in which they are immersed. If we do not obtain this result, it is a proof of the insufficiency of this cause: if, on the contrary, the means employed for suppressing evaporation being such as not to disturb the exercise of the other functions of the animal, we perceive a cessation of the phenomenon, for which we are endeavouring to account; we may conclude, with equal reason, that it was owing to evaporation.

Experiments with this view

by Dr. Fordyce

and Dr. Crawford.

This mode of ascertaining the influence of evaporation in this phenomenon naturally offered itself to the minds of those, who have instituted inquiries on the subject. Some experiments have been tried with this view; but they have neither been numerous, nor very decisive. One was by Dr. Fordyce. This gentleman, having introduced a large quantity of aqueous vapour into a heated room, thought he perceived, that the heat incommoded him more, but that his temperature remained scarcely the less stationary. It is to be observed, that the time he passed in this room was too short, to heat very perceptibly a mass so considerable as that of the human body. No positive inference therefore can be drawn from this experiment; any more than from that, in which Dr. Crawford attempted to ascertain the influence of a warm bath on the temperature of a dog; the mode in which he measured this temperature being too inexact, and, besides, the effect of the water being capable of suppressing the cuticular evaporation merely, not the pulmonary. The experiments which the same gentleman tried with frogs, animals in which the pulmonary evaporation must be inconsiderable, would be more decisive, if the results he gives were agreeable to observation. But I have satisfied myself, that this is not the case. Numerous experiments,

Frogs acquire the temperature of water in which they are. periments, made with care, have convinced me, that frogs, constantly acquire the temperature of the water in which they are immersed, let its heat be what it may, and that in this respect there is no difference between dead and living frogs*.

New experiments made These are all the experiments, as far as I know, that have been tried to ascertain what would happen, when men or animals are exposed to a high degree of heat, without any evaporation being able to take place from the surface of their bodies. Their insufficiency is evident. New ones therefore cannot be uninteresting, and these I have attempted.

on the plan of Dr. Fordyce. With this view I had recourse to the means employed by Dr. Fordyce, but with this difference; instead of trying the experiments with man, I employed animals of small size, that their mass might be more quickly heated. In fact it is well adapted to the end proposed; for it is obvious, that if an animal be placed in air loaded with vapour, there can be no evaporation of the fluid exhaled either on the surface of its body, or from its lungs; and yet it may continue the exercise of its functions as freely as in dry air. The apparatus I employed allowed me to distribute the vapour pretty uniformly throughout the space occupied by the animals, and to regulate the quantity at pleasure. The following is a description of it.

The apparatus described. A A, Pl. X, fig. 1, is a box, about forty inches high, twenty broad, and sixteen deep: divided into two compartments by an open partition, shown at *n n* in the section, fig. 2. At *a a* is a door, sliding in a groove, and opening into the upper chamber; in which is a circular wicker cage, *i i* fig 2, with a door opening opposite that of the outer box. In this cage the animals were placed. A thermometer, with a very long stem and small bulb, is fixed in the centre of this cage. It is protected from injury by an open wicker case, *m m* fig. 2; and reaches above the top of the box, fig. 1, at *b*, where it is graduated. The vapour is generated in a small tin boiler C C, heated by means of the furnace

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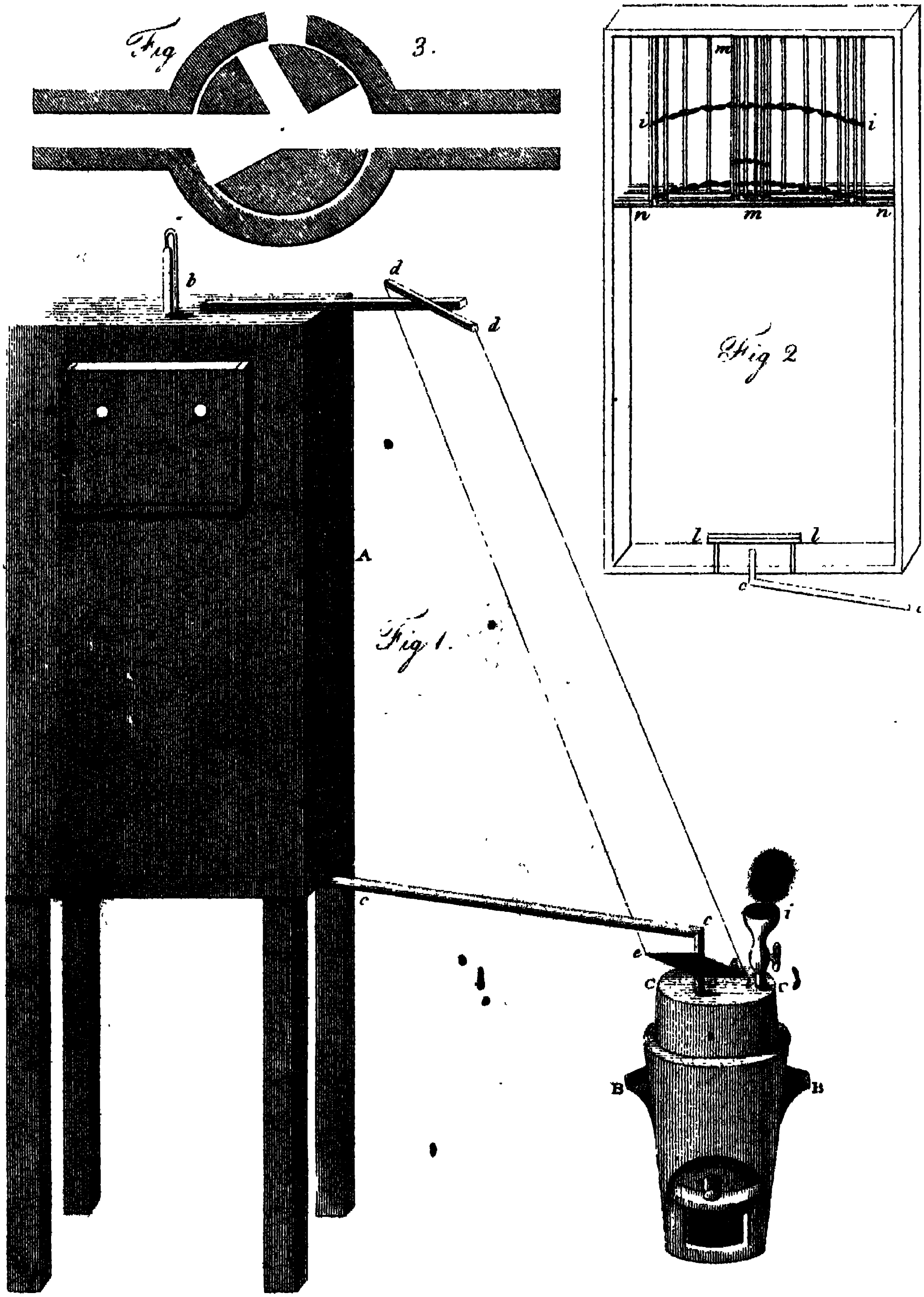
* See my Essay on the Effects produced in the Animal Economy by a high Degree of Heat, p. 54 and foll.

Delaroche's Apparatus for exposing Animals to heat.



furnace B B; and is conveyed into the box by a tube *c c*. Over the aperture of this tube, within the box, is placed at a little distance a plate *l l*, fig. 2, about four inches square; which prevents its ascending in a direct current to the top, and causes it to be distributed pretty uniformly throughout the apparatus. Toward the bottom of the tube of communication *c c* is a cock, so constructed, that the vapour may pass through a lateral opening, or through the tube itself, or through both at once. This allows the quantity of vapour admitted into the apparatus, and consequently the temperature, to be regulated at will. This is the more easy, as by means of a very simple contrivance, the key of the cock may be turned, without losing sight of the thermometer, that indicates the temperature of the box. To the key of the cock is affixed a pretty long lever, *e e*, from each end of which a string passes to the corresponding end of a lever of the same length, *d d*, turning on a pivot fixed on the top of the box. Of course by moving the lever *d d* a similar motion will be given to *e e*. A section of the cock is given at fig. 3. In the top of the boiler C C is a funnel with a cock, by means of which it can be replenished with water when necessary.

Experiments.

Into this apparatus I introduced successively warm-blooded animals of different kinds, and frogs: I exposed them to different degrees of heat: and I carefully examined their temperature, both before and after the experiment, by means of a thermometer introduced into the rectum, or plunged deep into the œsophagus. The results I obtained are given in the following table.

Results of the experiments.

TABLE of *Results obtained on exposing various Animals to a moist Heat, in order to determine the Influence of this Heat on their Temperature.*

Exp.	Animal.	Time of remaining in the box filled with vapour.	Mean temperature of the apparatus during the continuance of the experiment.		Temperature of the animal after its exposure to the vapour.		Temperature of the animal previous to its exposure.	
		Minutes.	Cent.	Fahr.	Cent.	Fahr.	Cent.	Fahr.
1	1st. rabbit	39	38·7°	101·66°	42·4	108·32°	40·0	104
2	Do.	55	38 7	101·66	43	109·40	39·6	103·28
3	Do.	52	40 7	105·26	43 6	110 48	40·0	104
4	2d rabbit	55	38·7	101·66	42·9	109·22	39·6	103·28
5	Do.	75	38·7	101·66	42·7	108·86	40·0	104
6	Do.	55	40·7	105·26	43 1	109·58	39·7	103·46
7	Guinea pig	56	37 7	99·86	42·7	108 86	39	102·20
8	Do.	55	38·7	101·66	42·9	109·22	39	102·20
9	Do.	48·5	40·7	105 26	43·5	110·30	39·0	102·20
10	Do.	55	40·7	105 26	44·2	111·56	38·4	101·12
11	Pigeon	55	37·7	99·86	43·8	110·84	42·5	108·50
12	Do.	40	40·7	105·26	45	113	41·9	107·42
13	Do.	42	41·9	107·42	46·9	116·42	41·8	107·24
14	1st. frog	73	25·6	78·08	26	78·80		
15	2d. frog	50	27·2	80·96	27·8	82·04		

I shall here subjoin some remarks with regard to the observations in this table. Remarks on them.

Beside the experiments, of which I have given the results, I made many others, on the accuracy of which I could not equally depend. Their results however were analogous to those in the table. Others made.

Whatever precautions I took to prevent it, there were always some variations in the temperature of the apparatus during the experiment. These variations did not in general exceed one degree [1·8° F]; but sometimes they amounted to 3° [5·4° F.], though for a very short period. The heat not quite constant.

When I employed the same animal in different experiments, I always suffered at least twenty-four hours to elapse between them. Animals.

The different thermometers I employed not being uniform in their motions, I examined these with care, and formed a particular scale of reduction for each, by means of which I reduced the several results of the different observations to one common scale. Though I have employed tenths of a degree in noting the results, I do not pretend to have been always thus exact in my observations; but I chose rather to express them thus, than to commit voluntary errours. The errours I have committed involuntarily however cannot at any time have exceeded a quarter of a degree. Thermometers

The temperature of an animal previous to its introduction into the apparatus has frequently exhibited trifling differences, the cause of which I could not ascertain. Previous temperature.

It was not easy to ascertain the temperature of the frogs immediately on their being taken out of the box, and without its being influenced by the contact of the hands or the external air. To effect this, I tied the animal on a kind of cart made for the purpose, and placed a thermometer with a very small bulb, so as to remain in its mouth, or rather in its stomach. On opening the box, I had only to take out the cart quickly, and examine the degree indicated by the thermometer. Management of the frogs.

On looking at the table we perceive, that the temperature of the warm-blooded animals uniformly rose two or three degrees at least above the moist air, in which they were immersed: whence it is evident, that the faculty of The temperature of the animals above that of the medium.

producing cold was annihilated in them, and consequently that this faculty is essentially dependent on evaporation. It is true, that the heat, to which these animals were exposed, did not exceed their natural temperature by two degrees [3·6° F.]; and it might be supposed, that the faculty of producing cold would have been displayed by them at higher temperatures. But this objection will vanish, if we consider, that death would have been the necessary consequence of their exposure for a time of any continuance to a moist heat, greater than that to which I subjected them, and consequently that this faculty would have been extinguished in them. In fact, however low the heat they endured in these experiments may appear to have been, they were always more or less exhausted by it; and when it was greater, they appeared dying on my taking them out of the apparatus. The guinea pig, though very lively in the morning, died in the evening after experiment 10. I had likewise a rabbit and a pigeon that died after similar experiments, the results of which are not inserted in the table.

They could not have borne a higher heat long.

It killed some.

It may be asked perhaps, why the temperature of these animals did not merely rise to an equilibrium with the surrounding fluid, instead of exceeding it by some degrees. The answer to this question is very simple. The exercise of their functions not having been disturbed, the cause, whatever it is, that produces animal heat, must have continued to act on them, and occasion this rise of temperature. It is more difficult to conceive, why this rise was not greater; and why the same cause, which in low temperatures keeps animals at 20°, 40°, or even 80° [36°, 72°, or 144° F.] above that of the surrounding air, does not rise more than 3° or 4° [5·4° or 7·2° F.], when they are exposed to heat*. This question cannot be solved, till we have a satisfactory answer to another of great importance, that has been often debated: "what is the cause of animal heat?"

Why did the heat of the animals increase.

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* Fresh experiments that I have made since this paper was read to the Institute, and which I shall soon make public, lead me to think that evaporation was not entirely suppressed in those I have here related: but these results, instead of invalidating what I have advanced, tend rather to confirm them.

a question which, as may be seen from the facts I have adduced, is not essentially connected with that here discussed.

Remark on frogs.

In frogs, and I believe it would be the same with other cold-blooded animals, the difference between their temperature and that of the surrounding medium was always much less striking than in warm-blooded animals, as might naturally be expected. This however has afforded me an opportunity of making a remark somewhat curious, but requiring to be confirmed by repeated experiments, namely, that the proper heat of these animals, or the excess of their temperature over that of the surrounding medium, is as considerable when they are exposed to heat, as when they are exposed to cold. This would seem to indicate, that the cause of this heat is not the same as in warm-blooded animals.

Curious fact respecting them.

The production of cold in animals owing to evaporation,

in which vital causes concur.

From what has been said we may conclude, that the production of cold, manifested in animals exposed to a high degree of heat, is to be classed with those phenomena, the essential cause of which is physical. In this however we cannot overlook the influence of vital causes, which, as I have announced at the commencement of this paper, concur with the physical causes in the production of almost all the phenomena, that are the result of organization. In fact, the evaporation, that causes this production of cold, cannot take place, unless the surface of the body and of the pulmonary cells be kept constantly moist. And here the comparison of inorganic bodies, such as were employed in my experiments, ceases to be exact. The surfaces of these were moistened by transudation only. Those of animals are moistened by perspiration, a very complex phenomenon, necessarily depend on the action of the system of capillary vessels. In the former, the surface no sooner begins to dry, than it draws from the interior a new portion of moisture. In the latter, on the contrary, perspiration must acquire a fresh degree of activity, when the heat becomes more considerable; and this can take place only from an increased energy in the exhalant system, and perhaps even throughout the whole of the circulation. It is to be observed, that this increased activity of the perspiration, at least from the surface of the body, is more considerable, than is requisite to furnish the increased evaporation. Hence that sweat, which, in most cases, is nothing more than the excess of the fluid perspired above that carried off by evaporation.

Sweat.

General deduction.

I shall conclude this paper with the following proposition, which I think I may venture to advance as a necessary consequence of the observations contained in it. "The production of cold, manifested in animals exposed to a high degree of heat, is the result of the evaporation of the perspirable matter; which, in consequence of the increased action of the exhalant system, is so much the more considerable, in proportion as the external heat is greater. It is therefore at the same time the result both of physical and vital causes."

VII.

A new and expeditious Mode of Budding. By THOMAS ANDREW KNIGHT, *Esq. F. R. S*[*].

Nurserymen apt to substitute one fruit for another.

PARKINSON, in his Paradisus Londoniensis, which was published in 1629, has observed, that the nurserymen of his days had been so long in the practice of substituting one variety of fruit for another, that the habit of doing so was almost become hereditary amongst them: were we to judge from the modern practice, in some public nurseries, we might suspect the possessors of them to be the offspring of intermarriages between the descendants of those alluded to by Parkinson. He has, however, mentioned his "very good friend, Master John Tradescant," and "Master John Miller," as exceptions; and similar exceptions are, I believe, to be found in modern days. It must however be admitted, that, wherever the character of the leaf does not expose the errour of the grafter, as in the different varieties of the peach and nectarine, mistakes will sometimes occur; and therefore a mode of changing the variety, or

Cause of mistake.

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[*] Transactions of the Hort. Soc. vol. i, p. 194.

of introducing a branch of another variety, with great expedition, may possible be acceptable to many readers of the Horticultural Transactions.

Expeditious mode of changing a variety.

The luxuriant shoots of peach and nectarine trees are generally barren; but the lateral shoots emitted, in the same season, by them, are often productive of fruit, particularly if treated in the manner recommended by me in the Horticultural Transactions of 1808 *. In the experiments I have there described, the bearing wood was afforded by the natural buds of the luxuriant shoots; but I thought it probable, that such might as readily be afforded by the inserted buds of another variety, under appropriate management. I therefore, as early in the month of June, of the year 1808, as the luxuriant shoots of my peach trees were grown sufficiently firm to permit the operation, inserted buds of other varieties into them, employing two distinct ligatures to hold the buds in their places. One ligature was first placed above the bud inserted; and upon the transverse section through the bark: the other, which had no further office than that of securing the bud, was applied in the usual way. As soon as the buds (which never fail under the preceding circumstances) had attached themselves, the ligatures last applied were taken off: but the others were suffered to remain. The passage of the sap upwards was in consequence much obstructed, and the inserted buds began to vegetate strongly in July: and when these had afforded shoots about four inches long, the remaining ligatures were taken off, to permit the excess of sap to pass on; and the young shoots were nailed to the wall. Being there properly exposed to light, their wood ripened well, and afforded blossoms in the succeeding spring: this would, I do not doubt, have afforded fruit; but that, leaving my residence at Elton, for this place, I removed my trees; and the whole of their blossoms, in the last spring, proved, in consequence, equally abortive.

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* Page 38: or Journ. vol. xviii, p. 196.

VIII. *Notice*

VIII.

Notice respecting Native Concrete Boracic Acid: By SMITHSON TENNANT, *Esq. F. R. S. &c. Communicated by* L. HORNER, *Esq. Sec. of the Geological Society* *.

Boracid acid found but in few places.

THE boracic acid is not found, like the greater number of substances, in almost every country; but, as far as our present knowledge extends, appears confined to a few particular places. On this account, as well as the great utility of borax in various arts, the discovery of its existence in any new situation may deserve to be recorded.

Volcanic product from Lipari.

Some months ago Mr. Horner was so obliging as to show me a collection of volcanic productions from the Lipari Islands, presented to the Geological Society by Dr. Saunders. They consisted chiefly of sulphur, and of saline sublimations on the lava; but among these more common substances there were several pieces of a scaly shining appearance, resembling boracic acid. The largest of these had been cut of a rectangular shape, and was about seven or eight inches in length, and five or six in breadth, as if it had been taken from a considerable mass. On one side of most of the pieces was a crust of sulphur, and the scaly part itself was yellower than pure boracic acid. To ascertain if the scaly part was coloured by sulphur, I exposed it to heat in a glass tube, and, after the usual quantity of water had come over, there sublimed from it about a tenth of its weight of sulphur, and the remainder was pure boracic acid.

Native boracic acid

mixed with a tenth of sulphur.

Another specimen.

Mr. Horner afterward informed me, that the late Dr. Menish of Chelmsford had presented to the Geological Society a specimen, which he had received, with some other volcanic productions, from Sicily, but which had been collected in the Lipari Islands; the box containing them being marked "Produzioni Volcaniche raccolte nelle Isole Eolie da Gius. Lazzari. Lipari." He found it to consist of boracic acid; and it perfectly resembled that I have just described, having the same yellow colour from an admixture of sulphur, and a similar crust of this substance adhering to one side.

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* Trans. of the Geol. Soc. vol. i, p. 389.

Any future traveller, visiting those countries, would do well to examine them with a view to this particular object. The boracic acid may be a more extensive volcanic product than has hitherto been imagined; for in the account given of its discovery some years ago, by Messrs. Hoefer and Mascagni, near Monte Rotondo, to the west of Sienna, we can have no doubt of its volcanic origin in those places, from the substances which are there described to accompany it.

Probably a less rare volcanic product than might be supposed.

IX.

Notice respecting the Decomposition of Sulphate of Iron by Animal Matter: by W. H. Pepys, *Esq. F. R. S. Treasurer of the Geological Society**.

As the following circumstance, that took place in my laboratory, appears to throw considerable light on the mode whereby organic remains become penetrated by pyrites, it may not perhaps be foreign to the objects of the Geological Society; and, as such, I have taken the liberty of offering it to their attention.

Mode in which organic matters are penetrated by pyrites

I was engaged a few years ago in a course of experiments on hidrogen gas, which was procured in the usual method, by the solution of iron turnings in diluted sulphuric acid. The sulphate of iron hence resulting, to the amount of some quarts, was poured into a large earthen pitcher, and remained undisturbed and unnoticed for about a twelvemonth. At the end of this time, the vessel being wanted, I was about to throw away the liquor, when my attention was excited by an oily appearance on its surface, together with a yellowish powder, and a quantity of small hairs.

A solution of sulphate of iron acted on by animal matter.

The powder, on examination, proved to be sulphur; and on pouring off carefully the supernatant liquor, there

Results.

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* Trans. of the Geol. Soc. vol. i, p. 399.

was discovered at the bottom of the vessel a sediment consisting of the bones of several mice, of small grains of pyrites, of sulphur, of crystallized green sulphate of iron, and of black muddy oxide of iron.

Part of the salt deoxigenated by it.

These appearances may with much probability be attributed to the mutual action of the animal matter and the sulphate of iron, by which a portion of the metallic salt seems to have been entirely disoxigenated.

X.

Analyses of Minerals: by MARTIN HENRY KLAPROTH, *Ph. D. &c.*

Analyses by Klaproth.

AS the fourth and fifth volumes of Klaproth's work have not appeared in English, and are not likely to be translated, the results of his analyses probably will not be unacceptable. Not having the work itself, they are taken from the *Journal de Physique.*

Electrum of Pliny.

Analysis of electrum.

This name is taken from Pliny, who says, book 33, chap. 4, § 23. "In all gold there is silver, in various proportions. Sometimes a tenth, sometimes a ninth, sometimes an eighth part. When there is a fifth part of silver, it is called electrum".

Gold ore of Schlagenberg.

In a gold ore from Schlagenberg in Siberia Klaproth found

Gold - - -	64
Silver - - -	36
	100

Silver ore from Peru.

From a species of silver ore, called in Peru *pacos*, some of which was brought over by von Humboldt, he obtained

Silver - - -	14
Brown oxide of iron -	71
Silex - - -	3·5
Sand - - -	1
Water - - -	8·5
	98

Conchoid muriated[*] silver from Peru. Another.

Silver - - -	67·75
Oxigen - - -	32·25
	100

Native cinnabar from Japan. Native cinnabar of Japan,

Mercury - - -	84·50
Sulphur - - -	14·75
	99·25

Native cinnabar from Neumarktel, in Carinthia, gave the same proportions. and of Neumarktel.

Hepatic cinnabar from Idria. Hepatic cinnabar of Idria.

Mercury -	81·80
Sulphur - -	13·75
Charcoal[†] -	2·30
Silex - -	0·65
Alumine - -	0·55
Oxide of iron -	0·20
Copper - -	0·02
Water - -	0·73
	100·

Red lamellar copper from Siberia. Red lamellar copper.

Copper - -	91
Oxigen - -	9
	100

Kupferlazur (radiated mountain blue) from Silesia. Radiated mountain blue.

Copper - - -	56
Oxigen - - -	14
Carbonic acid - -	24
Water - - -	6
	100

Malachite differs from this only in containing less carbonic acid, and more water. Malachite.

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[*] So in the Journ. de Physique. C.

[†] I do not know the word used by Klaproth: but as the French translator renders it *charbon,* not *carbon,* I have thought it best to employ charcoal. C

Kupfergruen

Mountain green.

Kupfergruen (mountain green) or chrysocol from Siberia.

Copper	40
Oxigen	10
Carbonic acid	7
Silex	26
Water	17
	100

Vitreous copper ore.

Kupferglanzerz (vitreous copper ore).

Copper	76·5
Iron	0·5
Sulphur	22
Loss	1
	100

Gray copper ore from Freyberg.

Fahlerz (gray copper ore) from Freyberg.

Copper	41
Silver	0·4
Arsenic	24·1
Iron	22·5
Sulphur	10
Loss	2
	100

Another.

Fahlerz from Kroner mine, Freyberg.

Copper	48
Silver	0·5
Iron	25·5
Arsenic	14
Sulphur	10
Loss	2
	100

Another.

Fahlerz from Jonas mine, Freyberg.

Copper	42·5
Silver	0·9
Iron	27·5
Antimony	1·5
Arsenic	15·6
Sulphur	10
Loss	2
	100

Crystallized graugultigerz from Kapnik.

Graugultigerz from Kapnik,

Copper - - -	37·75
Antimony - -	22
Zinc - - -	5
Iron - - -	3·25
Sulphur - - -	28
Silver - - -	0·25
Oxide of manganese -	
Loss - - -	3·75
	100

Graugultigerz in mass from Poratsch in Lower Hungary.

from Poratsch,

Copper - - -	39
Antimony - -	19·50
Iron - - -	7·50
Mercury - - -	6·25
Sulphur - - -	26
Loss - - -	1·75
	100

Graugultigerz in mass from Annaberg.

from Annaberg,

Copper - - -	40·25
Silver - - -	0·30
Antimony - -	23
Iron - - -	13·50
Sulphur - - -	18·50
Arsenic - - -	0·75
Loss - - -	3·70
	100

Crystallized graugultigerz from Zilla, in Clausthall.

from Zilla,

Copper - - -	37·5
Silver - - -	3
Antimony - - -	29
Iron - - -	6·5
Sulphur - - -	21·5
Loss - - -	2 5
	100

Graugultigerz

from St. Wenzel,

Graugultigerz from St. Wenzel, at Wolfach.

Coppper	25·50
Silver	13·25
Antimony	27
Iron	7
Sulphur	25·50
Loss	1·75
	100

and from Peru.

Graugultigerz in mass from Peru, brought over by Humboldt, and taken from the vein called Purgatorio.

Silver	10·25
Copper	27
Antimony	23·50
Iron	7
Lead	1·75
Sulphur	27·75
Loss	2·75
	100

Triple sulphuret of lead from Segen,

Ore of Antimony and lead from Segen, in Clausthal.

Lead	42·50
Antimony	19·75
Copper	11·75
Iron	5
Sulphur	18
Loss	3
	100

and from Andreaskreuz.

Ore of antimony and lead from Andreaskreuz, St. Andreasberg.

Lead	34·50
Silver	2·25
Copper	16·25
Antimony	16
Iron	13·75
Sulphur	13·50
Silex	2·50
Loss	1·25
	100

(*To be continued*).

SCIENTIFIC NEWS.

Wernerian Society.

At the meeting of this society on the 22d of February a communication from the Rev. Mr. Fleming of Flisk, was read, describing the mineralogical appearances, which occur on the north bank of the Frith of Tay, from Dundee up to Kingoodie quarry. The rocks are claystone, claystone-porphyry, felspar-porphyry, greenstone, sandstone and amygdaloid. The sandstone occurs in basin-shaped cavities in the porphyry, and contains subordinate beds of greenstone; but he deferred giving any decided opinion concerning the geognostic relations of these rocks, till he should examine the south shore of the Frith of Tay.

Mineralogy of the Frith of Tay,

At the same meeting, the secretary read a communication from Mr. Macgregor, surgeon to the 25th regiment, giving an account of the mineralogy of the country around the town of Lanark, particularly at the celebrated falls of Cora Lin and Stonebyres. Near the former, porphyry-slate and felspar-porphyry occur. At the latter, the waters are poured over beds of fine grained sandstone, which, in descending, gradually becomes coarser in texture, till it passes into a conglomerate, consisting of masses of quartz, jasper, splintery hornstone, flinty-slate, and clay-slate. Near Nethan bridge, the traces of a coal deposition and a portion of a coal-field make their appearance; many alternating beds of sandstone, bituminous shale, and clay ironstone occurring along with thin beds of slate coal and cannel coal. Mr. Macgregor stated it to be his opinion, that the sandstone exposed on the banks of the Clyde and of the Mouse river near Lanark, belongs to one and the same formation; that the Mouse has gradually scooped out the present channel, in the same way as the Clyde is supposed to have done; and that there are here no marks of any violent convulsion of nature, as some have imagined.

and of the country round Lanark

An extract of a letter from Lieutenant Huey of the 73rd regiment was also read, mentioning the circumstance of a large marine animal, supposed to be about thirty feet long, and shaped like a snake, having been observed from a ship in lat. 38° 13 S. and long. 5° E.

Large sea snake.

At

British sponges. At the meeting of this society on the 7th of March, the secretary read an "Essay on sponges, with descriptions of all the species, that have been discovered on the coast of Great Britain", by George Montagu, Esq. of Devonshire. Nature of sponge. From Mr. Montagu's researches as to the constitution of sponges, it appears, that no polypi or vermes of any kind are to be discerned in their cells or pores: they are, however, decidedly of an animal nature; but they possess vitality without perceptible action or motion. Mr. Montagu has divided the genus spongia into five families: viz. Branched, digitated, tubular, compact, and orbicular. Only fourteen species were previously known to be British: Mr. Montagu, in this communication, described no fewer than thirty-nine. A considerable number of the species are quite new, or have now for the first time been distinguished and formed by that indefatigable naturalist.

Natural method in botany. At the same meeting, Dr. Yule read a memoir on the natural method in botany, in which he defended the existence of the series of natural affinity in plants against the objections of professor Willdenow and Dr. Smith, founded on the want of regularity in the series, &c. He contended, that the illustrious author of the artificial system never intended, that it should supersede, but on the contrary, that it should lay the foundation of the Natural Classes, "quas plana genera nondum detecta revelabunt:" and that with this view he uniformly inculcated the study of natural genera, in conformity with his great maxim, "Omne genus naturale."

Literary and philosophical society of Liverpool. A literary and philosophical society has been lately instituted in Liverpool. Its object is to collect information in all branches of literature and science, which is laid before the society in the form of Essays, or Papers. The number of members already amounts to near sixty, and their meetings are held monthly from October to May inclusive. The communications and attendance are entirely voluntary. Officers; the Rev. Theophilus Houlbrooke, President: Rev. Joseph Smith, Dr. Bostock, and John Theodore Koster, Esq., annual Vice Presidents: and Dr. Tho. Stewart Traill, Secretary, to whom communications are to be addressed.

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